MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Dr. A. A. COMMON, LL.D., F.R.S., President, in the Chair.

Rev. T. Dunne Parker, LL.D., Bennington House, Stevenage, Herts;
John A. Ferguson, Newlands, Oak Hill, Surbiton, S.W.;
George Andrews Hill, United States Naval Observatory, Washington, D.C., U.S.A.; and
Edward William Hobson, Fellow of Christ's College, Cambridge,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended:—

Major Kingsley Foster, Shenley, Redhill (proposed by Sir R. S. Ball);
The Rev. Henry A. Hall, M.A., Head Master of the Grammar School, Totnes, South Devon (proposed by E. J. Stone);
Rhishard Llewelyn Jones, M.A. (Cantab.), Professor of Physics, The Presidency College, Madras (proposed by C. Michie Smith); and
Herbert Savery, M.A., Assistant Master at Marlborough College, Wilts (proposed by R. A. Sampson).

Eighty-three presents were announced as having been received since the last meeting, including, amongst others :—

Th. Brédikhine, Mouvement des substances émises par les Comètes 1893 II. et IV. ; H. S. Davis, Parallax of η *Cassiopeiæ*, deduced from the Rutherfurd photographic measures, presented by the authors ; Potsdam Observatory Publications, vol. vii. pt. 2 (Scheiner, Spectra der helleren Sterne), and vol. x. (Spoerer, Beobachtungen von Sonnenflecken), presented by the Observatory ; The Calendarium of Mohammed Al Achsasi Al Mouakket (Arabic MS.), presented by Mr. Knobel.

Reproduction of Astronomical Photographs.

The Council have recently decided to undertake the reproduction (by paper prints and lantern slides) of a selection of the Astronomical photographs now in the possession of the Society, or which may be submitted to the Society for the purpose.

To include any large number of such reproductions in the ordinary publications of the Society, which are distributed gratis, would not be possible unless the subscription were raised. Again, the selection of a small number from the wealth of excellent photographs now being produced would be not only invidious but extremely difficult.

In view of these considerations it seemed the easiest course to try how far the sale of such prints at approximately cost price would satisfy the requirements of astronomers. The Society would thus act as a convenient centre for receiving photographs intended for reproduction and distributing the prints, but would not be interested financially in the undertaking.

In accordance with the decision of Council several photographs have been already reproduced. The list given below shows those of which copies are now on sale ; others will follow. The prints are in two styles—platinotype and aristotype—mounted on sunk, cut-out mounts measuring 12 by 10 inches, and the price has been fixed at 1*s.* 6*d.* each.

In ordering prints it will only be necessary to quote the R.A.S. reference number. Orders to be addressed to W. H. Wesley. Full details as regards subject, instrumental data, exposure, &c., will be found on the back of each print. A set of specimen prints may be seen in the Library.

Suggestions will be gratefully received, either as to subjects of which reproduction is desirable or as regards any of the details.

The following prints are now on sale :—

R.A.S. Reference No.	Subject.	Photograph by
1	Total Solar Eclipse, 1889 January 1	W. H. Pickering
2	Total Solar Eclipse, 1893 April 16	J. M. Schaeberle
3	Total Solar Eclipse, 1886 August 29	A. Schuster
4	Nebulæ in the Pleiades	Isaac Roberts
5	Nebula M. 74 Piscium	Isaac Roberts
6	Great Nebula in Orion	Isaac Roberts
7	Milky Way near Messier II.	E. E. Barnard
8	Milky Way near Cluster in Perseus	E. E. Barnard
9	Comet *c* 1893 IV. (Brooks)	E. E. Barnard
10	Comet *a* 1892 I. (Swift)	E. E. Barnard
11	Nebula about η Argûs	David Gill
12	Portion of Moon (Hyginus-Albategnius)	MM. Lœwy and Puiseux

Arrangements are also being made for the supply of lantern slides prepared from the photographs in the possession of the Society. Further particulars respecting these will be issued as soon as the arrangements are complete.

A Determination of the Mean N.P.D. 1790 *January* 0, *of γ Draconis from Observations made at Oxford by Dr. Hornsby.* By E. J. Stone, M.A., F.R.S., Radcliffe Observer.

The Radcliffe Observatory, Oxford, was built about the year 1771, by the Radcliffe Trustees at the request of the University authorities, and furnished with the best instruments available at the time.

The equipment for the meridian observations consisted of two brass quadrants of 8 feet radius, a zenith sector of 12 feet focal length, and a transit instrument with an object glass of 4 inches. The mechanical parts of these instruments, and the divisions, were the work of the celebrated mechanician John Bird, and the optical work was by Dollond. There was no observatory in the world better equipped with meridian instruments than the Oxford Observatory at its foundation.

The quadrant and transit observations made by Dr. Hornsby in the year 1774, and in many subsequent years, were copied by him into books; but the papers generally are in great confusion. Dr. Hornsby reduced some selected observations for special purposes; but the observations have never been regularly reduced, nor, so far as I can find, have they been published in any form which would render them available for the use of astronomers.

The completion of the "Radcliffe Catalogue of 6,424 Stars for 1890" has afforded me the necessary leisure to take into con-

sideration the desirability of reducing some of the old observations made at the observatory.

An examination and partial reduction of the observations made with the quadrant and transit in the year 1774 have satisfied me that the observations are of very considerable accuracy.

I have therefore determined to completely reduce the observations of the year 1774 as a test of the expediency of a more extended reduction of these back observations. There are many considerations which appear to render these reductions desirable. We are relying for our determinations of many of the more important constants of astronomy upon the freedom of the observations made by Bradley at Greenwich, 1750–1762, from serious systematic errors; and our confidence is probably justifiable. But it was clearly shown by Le Verrier in his discussion of the solar observations that, when accurate meridian work was commenced at Paris about 1801, and at Königsberg about 1816, the results obtained were not in satisfactory agreement with the corresponding Greenwich results; and, if the Greenwich results were not then free from serious systematic errors, the question naturally arises, Up to what epoch are we justified in accepting them as free from such errors?

It appeared to me that some light might probably be thrown upon this question by a reduction of the observations made at Oxford about 1774; and, as an additional inducement to undertake the labour of these reductions, I find that a considerable number of observations of the Moon were made at Oxford about this period, which is a critical one in the lunar theory from the magnitude at that time of the doubtful theoretical inequality which Hansen has introduced in the construction of his Lunar Tables.

But as a preliminary step to the complete reduction of the quadrant observations, it has been thought desirable to examine the values of the zenith distances of *γ Draconis* at Oxford for the meridian transits which result from the zenith-sector observations; and the results thus obtained are given in the present paper.

It may be mentioned that the zenith-sector micrometer has two scales—one giving the number of completed revolutions up to eighteen, the other the divisions and decimal parts of a division—and that Bird cut the screw so that one revolution was very nearly equivalent to $27''$ of the sector arc. This equivalence was very nearly exact when the instrument was first mounted and adjusted by Bird, and it has been assumed to be exact by Hornsby in his reductions; but I have corrected the results for run whenever readings at consecutive three-minute divisions of the sector arc have been available.

I have also reduced all the observations with the values of the coefficients of aberration, nutation, and precession which are at present usually employed. A few sector observations were made in

1775 with a non-achromatic object glass: I have not at present reduced these observations. In 1777 an achromatic object glass by Dollond was mounted on the zenith sector, and a few observations were made in 1777 and 1778. I have reduced these observations. But during this period the observations were made with a direct eye-piece instead of with a far more convenient diagonal eye-piece first introduced in April, 1778. The desirability of measuring the star's ZD from two adjacent divisions of the sector arc was not apparently so fully recognised as was subsequently the case; the collimation error was unduly large; and there are no sufficient checks upon the constancy of the collimation error, sector east and west, for the observations which have to be combined. The line of collimation was considerably altered by Hornsby in 1778, when he introduced the diagonal eye-piece.

In the reduction of these sector observations it is necessary to assume that the line of collimation remains sensibly unchanged during the whole period for which the sector observations, east and west, are combined, and the only proof of this assumed constancy of the line of collimation is the agreement of the results thus obtained. It would have been more satisfactory if the zenith sector had been reversed oftener than was the case, but a combination of all the observations made, 1788–1791, shows that the changes of the line of collimation of the Oxford zenith sector must have been confined within small limits during this period; and the results thus obtained would, therefore, appear to be entitled to greater weight than those given by the observations 1777–1778, when the constancy of the line of collimation was more doubtful.

The latitude of the zenith sector is very nearly the same as that of the transit circle of the observatory; and if we assume the colatitude to be

$$38^\circ\ 14'\ 24''{\cdot}61,$$

and the proper motion of γ *Draconis* in N.P.D. to be $+0''{\cdot}028$, we deduce from the sector observations the following values for the mean N.P.D. 1790 January 0 :—

	From Observations.	No. of Obs.	Weight.	Mean N.P.D.
	Sector East.			
1	1788 Oct. 7 to Dec. 31	13	10·5	38° 28′ 50″·92
2	1789 Feb. 12 to Feb. 15	2	2	49·48
3	1791 Jan. 4 to Feb. 28	12	11	48·89
4	1791 Sept. 10 to Sept. 25	9	9	50·64
5	1791 Oct. 23 to Dec. 28	7	7	50·50
	Sector West.			
6	1789 Jan. 1 to Feb. 10	14	13·5	44·81
7	1790 Oct. 15 to Dec. 30	15	15	44·91
8	1791 Sept. 26 to Oct. 19	12	12	45·59

These separate results show that no great changes in the line of collimation took place between 1788 October 7 and 1791 December 28; and if we assume the collimation error to be sensibly constant for this period, we find for the mean collimation error, c, and the N.P.D. of γ *Draconis* the following values :—

Mean N.P.D. of γ *Draconis* 1790 Jan. 0 = 38° 28′ 47″·61 $\quad c = +2''{\cdot}52$

and the separate residuals, which include errors of observation and changes of the line of collimation from the mean line are—

	Sector East.		*West.*
1	+0″·79	6	−0″·21
2	−0·65	7	−0·18
3	−1·24	8	+0·50
4	+0·51		
5	+0·37		

The observations made, 1777–1778, lead to a larger result for the N.P.D., viz.

$$\frac{E+W}{2} = 38° \ 28' \ 50''{\cdot}44.$$

If we combined this result with that deduced from the observations, 1788–1791, with the full weight due to the number of observations, we should find

$$\text{N.P.D.} = 38° \ 28' \ 47''{\cdot}95.$$

But, for the reasons given, this is certainly allowing too much weight to the earlier observations. The N.P.D. of γ *Draconis* given by Dr. Auwers in his re-reduction of Bradley's observations brought up to 1790 January 0 is

$$38° \ 28' \ 48''{\cdot}34;$$

and this result appears to me to agree as closely as could have been expected, with the instruments available, with that found from the Oxford observations, 1788–1791 :—

$$38° \ 28' \ 47''{\cdot}61.$$

In the following reductions, when the run over 3′ has not been found from the observations, the adopted quantities have been put in brackets, and only half-weight has been given to these results.

Observations of γ Draconis with the Zenith Sector.

Date.	Division.	Reading of Micrometer For Division.	For Star.	Nominal Seconds of Micrometer in Run over 3′.	Apparent Z.D. (Uncorrected for Refraction.)	Mean Z.D. for 1778 Jan. 0.
	′	r d	r d		° ′ ″	° ′ ″
			Sector E.			
1777. Nov. 7	12	3 21·9	17 20·5	(180·0)	0 13 49·40	0 14 5·17
Dec. 16	12	10 4·5	5 17·5	179·7	0 14 2·20	5·91
	15	3 13·8				
27	12	8 9·0	3 15·5	180·8	7·92	7·27
	18	12 25·4				
29	12	13 0·0	8 5·5	179·6	9·79	8·41
	15	6 9·4				
1778. Jan. 21	12	8 0·5	2 22·5	(180·1)	19·92	10·58
22	12	15 10·5	10 6·2	180·3	19·07	9·41
	15	8 19·2				
Feb. 28	12	15 25·7	10 13·3	(182·0)	25·78	7·68
			Sector W.			
1778. Jan. 25	15	12 15·6	11 17·6	(181·8)	0 14 35·25	0 14 24·68
26	12	7 25·5	13 17·3	181·3	32·70	21·82
	15	14 17·8				
Feb. 13	12	7 14·0	13 16·9	181·9	43·18	27·64
	15	14 6·9				
14	12	3 23·2	9 23·2	181·3	40·84	25·08
	15	10 15·5				
17	15	3 18·7	3 1·0	(181·8)	42·48	26·12
23	15	9 1·0	8 12·3	(181·8)	44·46	27·06
25	12	7 3·8	13 9·0	182·6	44·82	27·12
	15	13 24·4				

Between 1778 Jan. 26 and Feb. 9 the sector was reversed from west to east and again brought back to position west; but no observations of γ *Draconis* were made in position east. The line of collimation would almost appear to have been shifted at the time by Dr. Hornsby, although I can find no notice of any such change.

1778 Feb. 28. The runs have been adopted from observations of α *Cygni* after the reversal from position west on Feb. 25.

A diagonal eye-piece was mounted on 1778 April 9; the observations before this period were made with a direct eye-piece.

Date.	Division.	Reading of Micrometer For Division.	Reading of Micrometer For Star.	Nominal Seconds of Micrometer in Run over 3′.	Apparent Z.D. (Uncorrected for Refraction.)	Mean Z.D. for Jan. 0.
			Sector E.			
1788.	′	r d	r d		° ′ ″	° ′ ″
Oct. 7	12	6 22·7	2 5·5	(182·5)	0 14 3·48	0 14 24·43
8	12	6 25·5	2 9·2	181·9	3·00	23·87
	15	0 5·6				
9	12	7 0·0	2 8·9	184·7	2·89	23·67
	15	0 4·3				
Dec. 4	12	5 15·8	0 11·1	(182·7)	17·64	26·52
12	12	11 12·5	6 2·1	(182·7)	23·25	29·36
15	12	11 10·8	6 3·6	(182·7)	20·10	25·20
17	12	17 10·1	12 5·9	182·8	17·07	21·50
	15	10 16·3				
21	12	17 12·5	12 4·5	181·3	21·97	24·68
	15	10 20·2				
*22	12	17 14·8	12 5·6	(181·5)	23·01	25·38
23	12	17 14·3	12 1·6	181·8	26·24	28·26
	15	10 21·5				
26	12	17 12·9	12 2·8	182·1	23·43	24·42
	15	10 19·8				
27	12	17 14·1	12 2·9	182·5	24·20	24·84
	15	10 20·6				
31	12	7·1	12 24·0	180·8	24·46	23·72
	15	11 15·3				
			Sector W.			
1789.						
Jan. 1	12	2 6·7	7 14·3	182·0	0 14 21·03	0 14 20·35
	15	8 26·7				
7	12	2 11·0	7 16·4	180·5	20·01	17·29
	15	9 2·5				
†11	12	2 11·8	7 17·2	173·7	25·49	21·45
	15	8 23·5				
13	12	2 5·4	7 17·3	182·9	24·57	19·87
	15	8 26·3				
16	12	2 4·4	7 14·7	182·4	23·39	17·74
	15	8 24·8				

* On 1788 Dec. 22 an observation was made with reference to division 18 for which the reading for the division was entered as $4^r\ 19^d{\cdot}0$. There is little doubt that the micrometer has been read backwards, and should be $4^r\ 8^d{\cdot}0$. It has been thought better to reject the observation than to make this conjectural alteration.

† 1789 Jan. 11. The observation is correctly reduced from Dr. Hornsby's MS., but the run appears unusually small.

Date.	Division.	Reading of Micrometer For Division.	For Star.	Nominal Seconds of Micrometer in Run over 3′.	Apparent Z.D. (Uncorrected for Refraction.)	Mean Z.D. for Jan. 0.
1789.	′	r d	r d		° ′ ″	° ′ ″
Jan. 18	12	2 5·2	7 15·7	181·3	0 14 24·46	0 14 18·19
	15	8 24·5				
30	15	8 14·7	7 9·4	(181·5)	27·97	18·24
Feb. 1	12	1 22·3	7 11·6	181·5	30·05	19·80
	15	8 14·8				
2	12	1 21·4	7 10·2	182·8	28·49	17 98
	15	8 15·2				
4	12	1 23·9	7 15·4	183·0	30·98	19·97
	15	8 17·9				
5	12	1 24·6	7 17·2	183·4	31·73	20·48
	15	8 19·0				
8	12	1 24·0	7 16·7	183·2	32·00	20·06
	15	8 18·2				
9	12	1 25·0	7 17·9	183·1	32·28	20·12
	15	8 19·1				
10	12	1 24·8	7 18·3	183·5	32·53	20·16
	15	8 19·3				
		Sector E.				
1789.						
Feb. 12	12	8 7·5	2 10·9	182·3	0 14 36·60	0 14 23·81
	15	1 14·2				
15	12	8 3·9	2 5·3	183·2	37·80	24·43
	15	1 9·7				
		Sector W.				
1790.						
Oct. 15	12	9 9·7	13 17·1	181·6	0 13 54·38	0 14 19·43
	15	16 2·3				
16	12	9 13·7	13 21·1	181·0	54·77	19·69
	15	16 5·7				
Nov. 4	12	9 6·7	13 19·0	182·2	58·85	20·46
	15	15 26·9				
5	12	9 6·2	13 18·3	182·4	0 13 58·52	19·90
	15	15 26·6				
13	12	9 3·1	13 17·3	182·3	0 14 0·66	20·13
	15	15 23·4				
15	12	9 5·7	13 20·9	183·5	0·85	19·80
	15	15 27·2				
16	12	9 5·7	13 21·8	183·1	2·00	20 69
	15	15 26·8				

Date.	Division.	Reading of Micrometer For Division.	Reading of Micrometer For Star.	Nominal Seconds of Micrometer in Run over 3′.	Apparent Z.D. (Uncorrected for Refraction.)	Mean Z.D. for Jan. 0.
1790	′	r d	r d		° ′ ″	° ′ ″
Nov. 27	12	9 3·7	13 22·4	182·1	0 14 5·24	0 14 20·61
	15	15 23·8				
*Dec. 6	12	16 26·7	3 18·8	183·0	5·02	17·69
	15	5 20·7				
12	12	10 22·0	15 20·0	184·3	9·90	20·57
	15	17 17·3				
20	12	0 6·3	5 5·8	181·7	13·24	20·81
	15	6 26·0				
21	12	7 19·5	12 20·2	182·9	13·55	20·78
	15	14 13·4				
†22	12	17 24·4	4 23·9	181·4	13·46	20·34
	15	6 16·8				
28	12	0 0·0	5 4·1	181·9	17·65	22·43
	15	6 19·9				
30	12	7 3·2	12 7·3	182·7	17·04	21·12
	15	13 23·9				
		Sector E.				
1791.						
Jan. 4	12	6 19·4	1 9·0	182·4	0 14 23·49	0 14 26·65
	15	17 26·0				
7	12	8 18·6	3 9·8	181·2	22·85	24·93
	15	1 26·4				
14	12	3 8·3	15 23·7	181·0	25·79	25·54
	15	14 16·3				
19	12	10 5·0	4 18·7	181·2	27·32	25·48
	15	3 12·8				
20	12	10 6·6	4 19·6	181·9	27·44	25·28
	15	3 13·7				
27	12	14 7·0	8 17·6	181·4	30·23	26·00
	15	7 14·6				
31	12	4 2·0	16 12·7	181·1	30·38	25·05
	15	15 9·9				
Feb. 2	12	15 20·4	10 3·7	181·1	30·78	24·93
	15	9 1·3				

* 1790 December 6, the reading of the micrometer has been diminished from $17^{r}\ 26^{d}{\cdot}7$ to $16^{r}\ 26^{d}{\cdot}7$.

† 1790 December 22, the reading of the micrometer has been diminished from $5^{r}\ 23^{d}{\cdot}9$ to $4^{r}\ 23^{d}{\cdot}9$.

Date.	Division.	Reading of Micrometer For Division.	For Star.	Nominal Seconds of Micrometer in Run over 3′.	Apparent Z.D. (Uncorrected for Refraction.)	Mean Z.D. for Jan. 0.
1791	′	r d	r d		° ′ ″	° ′ ″
Feb. 8	12	15 17·7	10 0·0	181·1	0 14 31·77	0 14 24·45
	15	8 25·6				
17	15	8 25·0	9 24·2	(181·8)	34·01	24·85
	15	8 24·5				
23	15	8 24 6	9 23·1	(181·8)	33·91	23·77
	15	8 22·9				
28	12	15 19·3	9 25·7	182·5	33·47	22 68
	15	8 25·8				
		Sector E.				
Sept. 10	12	5 3·3	0 20·1	181·9	0 13 56·97	0 14 25·06
	15	16 10·4				
11	12	5 5·5	0 21·1	183·2	57·31	25·46
	15	16 11·3				
12	12	5 5·4	0 23·1	179·7	57·50	25·70
	15	16 14·7				
13	12	5 7·0	0 21·7	183·1	58·26	26·51
	15	16 12·9				
14	12	5 6·2	0 20·8	181·7	59·27	27·56
	15	16 13·5				
15	12	5 7·5	0 23·1	181·2	58·61	26·93
	15	16 15·3				
16	12	5 7·0	0 20·8	181·0	60·53	28·88
	15	16 15·0				
18	12	5 9·0	0 24·5	181·0	58·84	27·23
	15	16 17·0				
25	12	5 8·7	0 24·0	180·5	59·37	27·73
	15	16 17·2				
		Sector W.				
Sept. 26	12	5 25·1	10 6·4	182·9	0 13 54·46	0 14 22·79
	15	12 19·0				
28	12	5 25·0	10 5·6	182·3	54·14	22·40
	15	12 18·3				
29	12	6 0·4	10 7·3	182·1	53·58	21·79
	15	12 20·5				
30	12	6 0·0	10 7·7	182·4	54·18	22·34
	15	12 20·4				

Date.	Division.	Reading of Micrometer For Division. r	d	For Star. r	d	Nominal Seconds of Micrometer in Run over 3′.	Apparent Z.D. (Uncorrected for Refraction.) °	′	″	Mean Z.D. for Jan. 0. °	′	″
1791	′											
Oct. 1	12	6	0·3	10	7·9	183·0	0	13	53·70	0	14	21·81
	15	12	21·3									
5	12	5	25·2	10	5·3	182·3			53·65			21·48
	15	12	18·5									
12	12	5	24·9	10	5·5	182·4			54·08			21·19
	15	12	18·3									
13	12	5	25·5	10	6·6	182·2			54·70			21·68
	15	12	18·7									
15	12	5	24·0	10	4·5	182·7			53·79			20·51
	15	12	17·7									
17	12	5	23·1	10	3·9	181·1			55·10			21·53
	15	12	15·2									
18	12	5	19·6	10	0·8	180·9			55·62			21·90
	15	12	11·5									
19	12	5	20·2	10	1·6	181·8			55·25			21·37
	15	12	13·0									
						Sector E.						
Oct. 23	12	14	23·4	10	11·0	178·5	0	14	1·41	0	14	26 84
	15	8	6·9									
24	12	14	22·5	10	7·8	180·6			2·29			27·54
	15	8	3·9									
27	12	14	24·8	10	9·3	182·1			2·08			26·75
	15	8	4·7									
Nov. 7	12	15	2·2	10	10·3	181·3			5·99			28·16
	15	8	9·9									
18	12	14	24·4	10	5·4	180·3			6·79			25·95
	15	8	6·1									
29	12	14	21·2	9	25·0	179·2			11·79			27·54
	15	8	4·0									
Dec. 28	12	14	22·1	9	18·7	180·1			18·32			23·72
	15	8	4·0									

On the Angular Distance of Two Stars in the Pleiades suitable for Determining the Value of a Micrometer Screw. By Professor H. H. Turner, M.A., B.Sc.

1. The following discussion was made in response to a request by Mr. H. F. Newall in 1895 February. He wrote : "I want to determine as accurately as possible the value of my micrometer screw, and have measured two stars in the *Pleiades* about 300″ apart. . . . Can you give me their accurate difference of Right Ascension ? To determine this by transits would involve much labour." It is possible that the results of the investigation may be useful to others also.

2. A long series of photographs of the *Pleiades* was taken at this observatory in 1892–93, and some measures on three of these plates have been published (*Monthly Notices*, liv. p. 489). To give a better determination, however, twelve additional plates were measured, as described below.

3. The stars used by Mr. Newall are those numbered 33 and 44 by Elkin.* His numeration will be hereinafter adopted. They are numbered 34 and 45 by Jacoby.†

The second, but not the first, was measured by Bessel in 1840, and called Anon. 24 ; and again by Pritchard (*Mem. R.A.S.*, vol. xlviii.) ; but neither by Ambronn, of Göttingen, in 1894. Adopting the precessions and secular variations given by Elkin to bring up both his own and Jacoby's places to 1892·0 (the equinox used for the Oxford photographic measures), we find the differences of Right Ascension and Declination are as follows :—

	R.A.	Decl.
Jacoby	$+4^{m}\ 50^{s}\cdot13$	$+44''\cdot74$
Elkin	$+4\ 49\cdot80$	$+44\cdot75$

The Oxford measures already published were made on plates taken on 1892 December 12, 13, and 1893 February 10 (*Monthly Notices*, liv. p. 489), and are expressed in rectangular coordinates on a tangent plane to the celestial sphere at *Alcyone*. It will be convenient to use these coordinates in what follows. The coordinates of the two stars on this plane with axes oriented for 1892·0, and expressed in *réseau* intervals (of 5′ of arc) are, according to Elkin's measures :—

* *Transactions of the Yale University Observatory*, vol. i., part 1, pp. 86 and 87.

† *The Rutherfurd Photographic Measures of the Group of the Pleiades. Observatory of Columbia College.* New York, 1892.

	Star.	ξ	η
	33	−0·8864	+2·0502
	44	−0·0034	+2·1990
Diff.	(44–33)	+0·8830	+0·1488

Jacoby's results, as well as those which follow, may be exhibited as corrections to these differences. The corrections indicated by Jacoby's measures in 1873·0 are—

ξ	η
+·0010	·0000

The corrections indicated by the three Oxford photographs are—

Plate.	ξ	η
252	+·0006	+·0013
259	−·0025	+·0007
294	+·0017	−·0001
Mean	−·0001	+·0006

There seems, therefore, no doubt that the relative proper motion of the two stars is very small, though further measures are desirable.

4. Accordingly on 1895 February 16 I measured the images of eight stars (two images each) on each of eight plates. Two of the stars were the above pair, and the other six were distributed so as to give a good determination of the constants of the plates. The micrometer used was that arranged with a scale in the eye-piece, described in *Monthly Notices*, lv. p. 102. The corrections to the measures due to errors of orientation, &c., of the plate affect the two close stars in question by nearly the same amount; and hence the details of the corrections need not be here given. The method of reduction adopted (by rectangular coordinates) is fully described in *Monthly Notices*, liv. p. 489, and again in lv. p. 102.

5. In Table I., the fifth and ninth columns give the actually measured distances between the stars (parallel to the *réseau* lines, and expressed in *réseau* intervals) for the two images on each plate. Each distance is the difference between two single readings of the eye-piece scale when placed centrally on each star in turn. The third figure is estimated. The next columns give the correction for orientation to the "mean plate" as determined from the four stars numbered by Elkin 19, 41, 43, and 60; and after applying this correction the results, given in columns 7 and 11, are strictly comparable. The differences from the means shown in columns 8 and 12 are not larger than is to be expected, considering that the last digit is estimated, or rather is the difference of two estimations. The errors may be treated as accidental.

TABLE I.

Plate.	Date of Exposure.	Tel. E or W. of pier.	Hour Angle West.	Measured $x_{44}-x_{33}$.	Correction.	Corrected Difference.	Excess over Mean.	Measured $y_{44}-y_{33}$.	Correction.	Corrected Difference.	Excess over Mean.
	1892.		h								
218 (1)	Oct. 17	W.	−3·4	0·890	...	·890	−·001	·151	·000	·151	+·001
(2)			−3·3	·893	...	·893	+·002	·151	·000	·151	+·001
243 (1)	Nov. 29	W.	−0·5	·894	−·001	·893	+·002	·145	+·004	·149	−·001
(2)			−0·3	·895	−·001	·894	+·003	·145	+·003	·148	−·002
244 (1)	Nov. 29	W.	0·0	·891	...	·891	·000	·148	+·003	·151	+·001
(2)			+0·2	·890	...	·890	−·001	·146	+·003	·149	−·001
263 (1)	Dec. 23	W.	−1·3	·892	...	·892	+·001	·148	+·001	·149	−·001
(2)			−1·2	·891	...	·891	·000	·150	+·001	·151	+·001
	1893.										
280 (1)	Jan. 14	W.	+0·1	·889	+·001	·890	−·001	·155	−·006	·149	−·001
(2)			+0·3	·890	+·001	·891	·000	·156	−·006	·150	·000
291 (1)	Feb. 8	E.	+1·5	·889	+·001	·890	−·001	·152	−·004	·148	−·002
(2)			+1·6	·890	+·001	·891	·000	·154	−·004	·150	·000
294 (1)	Feb. 10	W.	+2·1	·890	...	·890	−·001	·149	+·002	·151	+·001
(2)			+2·1	·888	...	·888	−·003	·149	+·002	·151	+·001
306 (1)	Mar. 4	E.	+2·8	·891	...	·891	·000	·149	·000	·149	−·001
(2)			+2·9	·890	...	·890	−·001	.151	·000	·151	+·001
					Mean	·8909			Mean	·1499	

6. Proceeding now to compare the "mean plate" with the theoretical tangent-plane at *Alcyone*, we use the "standard coordinates" ξ and η of the same stars, as given in *Monthly Notices*, liv. p. 494; and with the notation of that paper we find

$$a = e = +\cdot0078,\ b = -d = \cdot0000 \text{ sensibly},$$

the values of a and e agreeing well with those found in the above paper, which is a satisfactory indication of the permanence of the scale-value on the plates.

The differences thus become

$$+\cdot8840 \text{ and } +\cdot1491$$

as compared with the

$$+\cdot8830 \text{ and } +\cdot1488$$

of Elkin, giving corrections to Elkin of

$$+\cdot0010 \text{ and } +\cdot0003.$$

7. It may be objected that these measures are made by a comparatively rough method; and this is to some extent true. On the other hand eight different plates were measured and two independent images on each, and the agreement of the different results is fairly satisfactory. Personally, having had some considerable experience of this particular form of micrometer, I am inclined to estimate its capabilities distinctly high; but, as others may not yet agree in this estimate, the following independent investigation was made for the x-coordinates.

Mr. G. F. H. Smith, of New College, Oxford, using the micrometer made by Messrs. Troughton & Simms for the late Professor Pritchard, which is provided with an excellent steel screw ($1^{rev} = 1^{mm} = 1'$ approx.), measured the difference of the x coordinates of the two stars 33 and 44 on twelve plates, eight of which were the same as the above. The plates were carefully oriented, so that the direction of measurement was parallel to one set of *réseau* lines, and the corrections for orientation already made use of were thus again available in the case of eight plates. For the others corrections were determined by a similar method. The only new constant required is the value of one revolution of the screw in terms of a *réseau* interval.

8. *Uniformity of Screw.*—The total available length of the screw is about 25 revs. Seven sets of comparisons of different portions of the screw with *réseau* intervals give the following results. It should be remarked that the eye-piece is fixed and the plate bodily moved by the screw, so that a change of focal distance does not affect the measures.

Value of one *réseau* interval in terms of revolutions of screw:

0^r — 5^r	— 10^r	— 15^r	— 20^r
$5^r\cdot0039$	$5\cdot0004$	$4\cdot9977$	$4\cdot9966$

There is apparently, therefore, a gradual change in the value of 1^{rev} as we proceed along the screw. In the star-measures care was taken always to use the same part of the screw (viz. the portion $5^r - 10^r$) for measuring the distance between the stars 33 and 44; or, in exceptional cases, to apply the corresponding correction.

9. *Value of* 1^{rev} *of Screw.*—This is not assumed to remain constant, since the expansion of the film is probably not the same as that of the screw. At the time of measuring the stars, three or four *réseau* intervals were measured with the screw also. When other parts of the screw than the portion $5^r - 10^r$ were used for either stars or *réseau* intervals, the measures were corrected according to the results of the last paragraph. The mean value of a revolution of the screw, thus found at the time, has been used to reduce the star-measures to *réseau* intervals. The measures are thus expressed in terms of the average *réseau* interval, which is the unit employed in connecting the measures on the plate with Elkin's measures in arc, according to the methods explained above.

10. In the following table, column 5 gives the measure in *réseau* intervals, assuming 5^{rev} of the screw to be equal to one interval. Column 6 gives the correction for runs as in the last paragraph; column 7 the corrections for orientation, &c., found as above, and supplied by Mr. G. F. H. Smith were necessary; column 8 the finally corrected distance; and column 9 the residuals.

Table II.

Plate.	Date of Exp.	Tel. W. or E. of pier.	Hour Angle. h	Measure.	Corr. for Runs.	Corr. for Orientation, &c.	Corrected Measure.	Excess over Mean.
218	Oct. 17	W.	−3·4	·8910	−·0011	−·0074	·8825	−·0009
243	Nov. 29	W.	−0·5	·8909	−·0005	−·0072	·8832	−·0002
244	,, 29	W.	0·0	·8911	−·0002	−·0074	·8835	+·0001
251	Dec. 12	W.	−2·6	·8912	−·0004	−·0070	·8838	+·0004
254	,, 12	W.	−1·5	·8908	−·0005	−·0066	·8837	+·0003
263	,, 23	W.	−1·3	·8915	−·0008	−·0070	·8837	+·0003
280	Jan. 14	W.	+0·1	·8914	−·0007	−·0058	·8849	+·0015
281	,, 14	W.	+0·7	·8900	−·0005	−·0070	·8825	−·0009
291	Feb. 8	E.	+1·5	·8902	−·0005	−·0062	·8835	+·0001
294	,, 10	W.	+2·1	8899	−·0003	−·0074	·8822	−·0012
296	,, 14	W.	+0·9	·8900	−·0002	−·0066	·8832	−·0002
306	Mar. 4	E.	+2·8	·8907	−·0004	−·0068	·8835	+·0002
						Mean	·8834	

The mean value thus gives a correction to Elkin of +·0004, as compared with +·0010 from the former measures and −·0001

from those previously published. The results for the different plates, being each the mean of several readings, naturally accord more closely than before.

11. As one or two of the residuals seem large, Mr. G. F. H. Smith repeated some of his measures; but it seemed clear that the discrepancies were accidental, and nothing is gained by adding these few new measures to the above.

12. Collecting all the results we thus have the following corrections to Elkin's heliometer measures from photographic measures

TABLE III.

Measurer.	Place.	Date of Photos.	Corrections to ξ	η
Rutherfurd and Jacoby	New York	1873·0	+·0010	·0000
(Elkin	Yale College	1885·0	·0000	·0000)
Bellamy	Oxford	1893·0	−·0001	+·0006
Turner	,,	,,	+·0010	+·0003
Smith	,,	,,	+·0004	...

Combining the Oxford results with equal weight we should obtain proper motions of −·00003 and +·00001 in ξ and η respectively from the extreme measures. Applying these to the two earlier results we get for the simple mean of New York, Yale, and Oxford, at epoch 1893·0,

+·0002 and +·0001,

or in the more familiar unit of seconds of arc

in R.A. +0″·06 and in Decl. +0″·03,

with the annual increments

−0″·009 and +0″·003 respectively.

13. It would thus appear that, so far as these measures indicate, Elkin's distance between these stars does not at the present time require a correction of so much as 0″·1. The following may be given as the definitive value of the distance in arc of a great circle, if it is preferred to use this instead of the difference of R.A.

$$268''\cdot71 - 0''\cdot008\,(t - 1893\cdot0).$$

A General Method for Facilitating the Solution of Kepler's Equation by Mechanical Means. By T. J. J. See, A.M., Ph.D. (Berlin).

The standard works on planetary motion, such as Gauss' *Theoria Motus*, Oppolzer's *Bahnbestimmung*, and Watson's *Theoretical Astronomy*, give methods for solving Kepler's equation which are very satisfactory when the eccentricity of the orbit is small, and also when this element is large, as in the case of most of the periodic comets. When the eccentricity is small, an expansion in series, usually by Lagrange's Theorem, enables us to find the eccentric anomaly with the desired facility. The series frequently employed has the form

$$E_0 = M + e'' \sin M + e'' \left(\frac{e}{2}\right) \sin 2M + \ldots .$$

To the approximate value E_0, obtained from a few terms of this series, we apply a correction resulting from the expansion by Taylor's Theorem :

$$E = E_0 + \frac{dE_0}{dM_0} dM_0 + \ldots .$$

The equation of Kepler gives

$$\frac{dM_0}{dE_0} = 1 - e \cos E_0 ;$$

and since

$$dM_0 = M - M_0,$$

we find two terms of the series to be

$$E = E_0 + \frac{M - M_0}{1 - e \cos E_0}.$$

Successive applications of this formula will readily yield the true value of the eccentric anomaly. But when the eccentricity is considerable the expansion in series fails to converge with the desired rapidity. On the other hand, when the orbits differ but little from parabolas, the solution can be readily found by means of special tables, such as those given by Gauss, Watson, and Oppolzer.

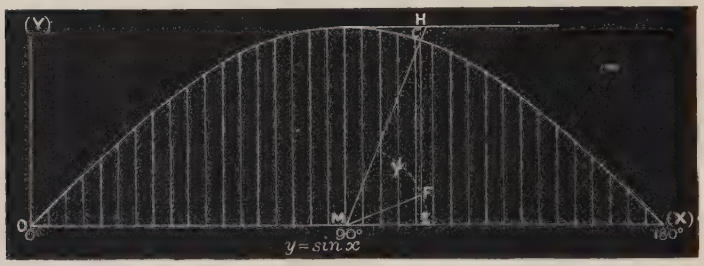
It is very remarkable that among the many solutions of Kepler's equation discovered by mathematicians there is not one, so far as I am aware, which has come into general use among astronomers that is applicable to ellipses of all possible eccentricities. The method to which I desire to call attention is a modification of the graphical method given by Klinkerfues,* and originally proposed by Dubois.†

* *Theoretische Astronomie*, p. 17.
† *Astronomische Nachrichten*, No. 1404.

Suppose we construct, on a convenient scale, a semicircumference of the curve of sines, $y = \sin x$. In practice it is desirable to use millimetre paper, and a convenient scale is obtained by taking one degree of the arc as five millimetres, so that the scale may easily be read to $0°{\cdot}1$. The origin of the arc is taken at the origin of coordinates; and as the scale along the axis of abscissæ extends from 0° to 180°, it will have a length of 90 centimetres.

In the figure let OM represent the mean anomaly, and suppose from M we draw a right line making an angle Ψ with the axis of abscissæ, the angle Ψ being defined by the equation

$$\tan \Psi = \frac{1}{e}.$$



Let the abscissa of the point C, determined by the intersection of the right line MC with the sine curve, be denoted by E. Then we evidently have

$$OE - ME = OM.$$

Thus, denoting the arc OE by E, and observing that $e \sin \Psi = \cos \Psi$, we find that $e \sin \Psi = ME$, the radius in case of $\sin \Psi$ being such that $\sin \Psi$ is always equal to $\sin E$.

Hence we get

$$OE - ME = OM,$$

or

$$E - e \sin E = M,$$

which is the equation of Kepler.

Therefore we conclude that if for an orbit of given eccentricity we construct a triangle CME (in practice this may be made of cardboard) and apply the vertex M of the triangle to the successive mean anomalies, the base coinciding with the x axis, the intersection of the hypothenuse with the curve of sines will give at once abscissæ which are the corresponding eccentric anomalies. Any actual diagram such as we have described will be subject to slight inaccuracies of construction owing to the transcendental nature of the sines, and hence we cannot obtain solutions of absolute precision. But it is entirely possible to get approximate solutions exact to $0°{\cdot}1$, and this work

can be done with the greatest rapidity. It is merely necessary to slide the base of the triangle along the x axis, placing the vertex M at the points corresponding to the different values of the mean anomaly, and reading off the corresponding eccentric anomalies.

This triangle device is rendered possible by virtue of the fact that Ψ is constant in $\tan \Psi = \frac{1}{e}$; and we may observe that in case of elliptic orbits the angle Ψ can only vary from 45° in case of a parabola to 90° in case of a circle. This method is therefore directly applicable to ellipses of every possible eccentricity, and the accuracy of the solution is always substantially the same. In case of parabolic motion, however, the method fails, since when $\Psi = 45°$ the hypothenuse MC is tangent to the sine curve at the origin. But for $e < 1$ the hypothenuse MC intersects the curve $y = \sin x$, and the intersection will be well defined except when e approaches unity and M is very small. In such cases it is best to use the Special Tables or the Theory of Parabolic Motion. Solutions exact to 0°·1 are often sufficient in the present state of double-star observation, and we readily see how great is the practical value of this method in comparing a long series of observations with a given set of elements. One hundred approximate solutions of Kepler's Equation, accurate to 0°·1, may be obtained by this method in less than half an hour; while if e lies between 0·35 and 0·85 a skilled computer could probably not obtain the same results by the ordinary method in less than a day. Thus the time and labour required for this work is much diminished, and it is clear that the chances of large error are correspondingly reduced.

If a curve of sines were engraved on a metallic plate it would be an easy matter to devise a movable protractor which could be set at any angle; such a piece of apparatus would serve for every possible elliptic orbit, and would last for an indefinite time. Considering the immense labour devolving upon astronomers in the computation of the motion of the heavenly bodies, it would seem that such a labour-saving device might be advantageously employed in the offices of the astronomical ephemerides. However, as several astronomers have prepared tables for facilitating the solution of Kepler's Equation in case of orbits which are not very eccentric, such an apparatus would be useful chiefly in work on the more eccentric asteroids, the double stars, and periodic comets. In dealing with the motions of these bodies the labour saved would be very considerable, and we might hope that the apparatus here suggested would come into actual use. But in case this instrument of precision could not be successfully manufactured, owing to its limited commercial use, it is easy for a working astronomer to construct a curve of sines on millimetre paper. This can be mounted on a suitable wooden board, and a triangle of cardboard will give the solutions of Kepler's Equation for any given orbit.

Klinkerfues has shown how we may use the diagram to correct our approximate values of E with great rapidity. Suppose we take a pair of dividers and set them to the value of e on the scale of the sine curve; then, placing one foot at M, let us describe a circular arc cutting the ordinate CE in F. The line $MF=e$, $ME=e \sin E$, and $FE=e \cos E$. Hence

$$HF = 1 - e \cos E = \frac{dM}{dE}.$$

The exact value of M is given, and since

$$M_0 = E_0 - e \sin E_0 = OE - ME,$$

the value of $dM_0 = M - M_0$ is at once obtained. Then we have

$$\Delta E_0 = \frac{M - M_0}{1 - e \cos E_0},$$

which gives a second approximation to the true value of the eccentric anomaly. This correction can be applied with great rapidity, since the dividers will always have the same setting for a given eccentricity.

Thus, while the graphical method, originally proposed by Dubois and afterwards improved by Klinkerfues, has been suggested many years ago, it does not appear that it has ever come into actual use; and I have therefore thought it worthy of the attention of astronomers, particularly in the modified form here proposed. For it seems clear that a general method for solving Kepler's Equation by mechanical means is an urgent *desideratum* of astronomy, owing to the large number of orbits requiring investigation and the wide range of their eccentricities.

In computing the motions of double stars, where the eccentricities have all values from 0·1 to 0·9, I have found this device of the greatest service. It will be almost, if not quite, equally important in case of the periodic comets and the asteroids. But in dealing with comets and planets, where we desire very exact solutions of Kepler's Equation, I would suggest that the approximate values of E should be read off, and one correction applied in the manner here indicated; a second correction to be *computed* by the formula

$$\Delta E_0' = \frac{M - M_0'}{1 - e \cos E_0'},$$

where M_0', E_0' are the first corrected values of E_0, or

$$E_0 + \frac{M - M_0}{1 - e \cos E_0}$$

will probably give the true values of the eccentric anomaly

$$E = E_0' + \Delta E_0',$$

within a small fraction of a second of arc. Thus practically all *computation* is done away with, except a single correction which would be required in order to insure the accuracy desirable in planetary and cometary ephemerides.

The University of Chicago:
1895 *May* 24.

On a Catalogue of Stars in the Calendarium of Mohammad Al Achsasi Al Mouakket. By E. B. Knobel.

I have lately obtained a 17th century Arabic MS. of a calendarium by Al Achsasi—a name that is not to be found in the usual sources of information about Oriental astronomers, such as D'Herbelot, *Bibliothèque Orientale*; Casiri, *Bibliotheca Escurialensis*; Houzeau, *Bibliographie de l'Astronomie*; the British Museum, the Bodleian, and the Leyden Libraries, &c. But fortunately I have found the name of the author and the title of the treatise in the catalogue of the Khedive's library at Cairo, but without any date.

The author, Mohammad Al Achsasi Al Mouakket, was Sheikh of the Grand Mosque at the College at Cairo, and surnamed Al Mouakket from his functions of regulating the times and the hours at the mosque. He derived his name "Achsasi" from a village so called, which I find existed in the province of Fayoum in Egypt. The MS. was therefore clearly written at Cairo. Its title is "Durret Al Muddiah Fih Al Aamal Al Shamsiah" (Pearls of brilliance upon the solar operations).

The MS. is unfortunately not complete, but it contains tables of the position of the Sun for each day of the Coptic and Syriac months, a list of cities in Arabia and Northern Africa, a concordance of Coptic and Mohammadan years, a catalogue of stars, a complete almanac of the Coptic year, and some other astronomical tables.

The catalogue gives the positions of 112 stars in right ascension and declination; and as it presents some points of interest I have translated this portion of the MS. as perhaps worthy of publication.

Prior to modern European astronomy it was unusual to give the positions of stars in right ascension and declination, and in the few cases where this was done by Oriental astronomers the right ascensions, designated by the term al matalia "coascendants," were always calculated from the first point of Capricornus —the solstitial colure, as we find in the present instance.

In investigating this MS. it is natural to suspect that it is a copy of the Calendarium of Al Tizini, who also bore the designation of "Al Mouakket," and who published his tables at Damascus in 1534. Al Tizini's catalogue of 302 stars was translated by

Hyde and published by him in 1665 as an appendix to his translation of Ulugh Beg. I have no doubt whatever that Al Achsasi based his tables upon those of Al Tizini; but the catalogue of the former contains a few stars not given by the latter, and also some differences in the descriptions of the stars, which indicate that it may not be simply a copy reduced to a later epoch.

The MS. is fairly written, but absolutely without vowels and very deficient in diacritical points, and there are numerous mistakes in words and figures.

The catalogue in the MS. gives the names of the stars, the right ascension, declination and magnitudes, and a column headed by a word which signifies the *nature* of the star. Against some stars are affixed certain letters, which probably indicate the Sun or some of the planets, and are clearly of astrological significance. Similar columns are to be found in the editions of Ptolemy's Catalogue published in the Alfonsine tables of 1483, the Trapezuntius Ptolemy of 1528, Bartsch's Planisphærium Stellatum, and many other mediæval works.

The epoch is not given, but the positions of the stars seem to be for about the year 1650, and on this assumption I append a comparison of the right ascensions of twenty-four stars in Al Achsasi and Al Tizini (reduced to 1650) with the correct positions for that epoch derived from Dr. Danckworth's catalogue published in the *Vierteljahrsschrift* for 1881.

In the catalogue I have given the Arabic names with the Latin translation and the modern designation of the stars.

No.	Arabic Name.	Modern Name.	Right Ascension.	Declination.	Region.	Mag.
1	Rabah al Waridah * *Quarta τοῦ al Warida*	η Sagittarii	0 41	36 5	S	3
2	Rai al Naaim *Pastor Struthionum*	λ Sagittarii	1 56	26 29	...	4
3	Aoul al Sadirah * *Prima τοῦ al Sadirah*	ϕ Sagittarii	6 0	26 27	S	1
4	Al Nesr al Waki * *Vultur Cadens*	α Lyræ	6 53	38 30	N	...
5	Thanih al Sadirah *Secunda τοῦ al Sadirah*	σ Sagittarii	8 44	26 48	S	3
6	Dzaneb al Heiyet * *Cauda Serpentis*	θ Serpentis	9 44	3 11	N	...
7	Thalath al Sadirah *Tertia τοῦ al Sadirah*	ζ Sagittarii	10 31	30 25	S	3
8	Nir al Beldat * *Lucida Oppidi*	π Sagittarii	11 18	21 55	S	3
9	Dzaneb al Tair *Cauda (Vulturis) Volantis*	ζ Aquilæ	12 27	13 0	N	3
10	Djenubi Menkib al Nesr *Australior Humerus Vulturis*	δ Aquilæ	17 21	2 27	N	3

No.	Arabic Name.	Modern Name.	Right Ascension.	Declination.	Region.	Mag.
11	Menchir al Dedjadjet *Rostrum Gallinæ*	β Cygni	20 26	26 21	N	2
12	Menkib al Nesr *Humerus Vulturis*	γ Aquilæ	22 32	9 36	N	2
13	Al Nesr al Tair * *Vultur Volans*	α Aquilæ	23 46	30 29	N	2
14	Unuk al Ghyrab * *Collum Corvi* (i.e., *Vulturis*)	β Aquilæ	25 50	5 41	N	3
15	Thanih Ras al Akab *Secunda Capitis Vulturis*	θ Aquilæ	28 19	3 0	S	3
16	Shimalih Saad al Dzabihh * *Borealior Fortunæ Immolatoris*	α Capricorni	30 28	13 33	S	3
17	Sadr al Dedjadjet * *Pectus Gallinæ*	γ Cygni	33 20	29 0	N	3
18	Dzaneb al Delphin *Cauda Delphini*	ε Delphini	35 35	10 1	N	4
19	Nir Saad Bula * *Lucida Fortunæ Dissipantis*	ε Aquarii	36 43	11 12	S	4
20	Nir Saad al Saaoud * *Lucida Fortunæ Fortunarum*	β Aquarii	49 2	30 23	S	2
21	Thanih Saad al Saaoud *Secunda Fortunæ Fortunarum*	ξ Aquarii	50 36	9 40	S	3
22	Fam al Feras *Os Equi*	ε Pegasi	52 38	8 33	N	3
23	Aoul al Achbiya * *Prima Tabernaculorum*	γ Aquarii	61 4	3 18	S	4
24	Wasat al Achbiya *Media Tabernaculorum*	π Aquarii	62 0	1 18	S	4
25	Achr al Achbiya *Postrema Tabernaculorum*	ζ Aquarii	64 40	2 0	S	3
26	Nir Saad al Bahaim * *Lucida Fortunæ Ferarum*	ζ Pegasi	66 21	8 30	N	3
27	Al Dsefadah al Aoul *Rana Prima*	α Piscis Australis	70 0	33 48	S	1
28	Shimalih al Mukdim * *Borealior*	β Pegasi	72 6	25 0	N	1
29	Djenubi al Mukdim *Australior*	α Pegasi	72 36	13 20	N	2
30	Fam al Samkat *Os Piscis*	β Piscium	75 1	1 0	N	4
31	Keff al Salsalat * *Manus Catenatæ*	ι Andromedæ	81 25	4 33	N	4
32	Dzaneb al Samkat *Cauda Piscis*	ω Piscium	85 36	4 32	N	...
33	Shimalih al Muchir * *Borealior*	δ Pegasi	88 0	26 13	N	2

No.	Arabic Name.	Modern Name.	Right Ascension.	Declination.	Region.	Mag.
34	Djenubi al Muchir *Australior*	γ Pegasi	89 20	14 32	N	2
35	Al Dsefadah al Thanih * *Rana Secunda*	β Ceti	96 55	18 0	S	3
36	Al Rascha * *Funis*	β Andromedæ	102 35	33 13	N	2
37	Aoul al Naamet * *Prima Struthionum*	η Ceti	103 0	11 30	S	2
38	Thanih al Naamet * *Secunda Struthionum*	θ Ceti	107 0	10 15	S	2
39	Thalath al Naamet * *Tertia Struthionum*	τ Ceti	112 0	18 30	S	2
40	Al Djedi * *Capra*	α Ursæ Minoris	105 17	?	N	2
41	Ras al Mathlath *Caput Trianguli*	α Trianguli	113 20	26 13	N	...
42	Rabah al Naamet *Quarta Struthionum*	ζ Ceti	114 35	12 0	...	2
43	Shimalih al Sheratein * *Borealior Signorum*	β Arietis	114 26	18 0	N	2
44	Khamis al Naamet * *Quinta Struthionum*	γ Andromedæ	115 36	40 8	N	3
45	Al Natih *Cornu*	α Arietis	116 0	20 30	N	2
46	Nir al Botein * *Lucida Ventris*	δ Arietis	136 21	23 0	N	4
47	Keff al Djezma * *Manus Truncata*	α Ceti	131 26	2 11	N	4
48	Achr al Nahr *Postrema Fluminis*	θ Eridani	132 0	41 0	S	1
49	Al Thaur al Thureiya ? *Pleiadum*	η Tauri	141 12	22 0	N	4
50	Sadr al Thaur *Pectus Tauri*	λ Tauri	145 37	11 3	N	2
51	Aoul al Debaran * *Prima τοῦ al Debaran*	γ Tauri	150 0	14 40	N	2
52	Nir al Debaran * *Lucida τοῦ al Debaran*	α Tauri	156 21	21 0	N	...
53	Wasat Taj al Djauza *Media Tiaræ Gigantis*	π Orionis	158 0	7 21	N	4
54	Rabah al Nahr *Quarta Fluminis*	ω Eridani	159 0	6 30	S	4
55	Al Aiouk * ?	α Aurigæ	162 0	44 30	N	4
56	Rijil al Djauza * *Pes Gigantis*	β Orionis	164 0	21 3	S	1

No.	Arabic Name.	Modern Name.	Right Ascension.	Declination.	Region.	Mag.
57	Kurn al Thaur al Shimalih *Cornu Boreale Tauri*	β Tauri	165 50	28 0	N	2
58	Menkib al Djauza al Aisr *Humerus Sinister Gigantis*	γ Orionis	166 5	5 32	N	2
59	Aoul al Nazhm *Prima Lineæ Margaritarum*	δ Orionis	168 46	1 11	S	2
60	Ras al Djauza *Caput Gigantis*	λ Orionis	170 0	9 36	N	Neb.
61	Rekbah al Djauza al Yemeniat* *Genu Dextrum Gigantis*	κ Orionis	173 0	10 11	S	2
62	Mirzem al Yemeniat * ?	β Canis Majoris	183 0	17 45	S	3
63	Nir al Henat * *Lucida τοῦ al Henat*	γ Geminorum	184 0	36 0	N	4
64	Al Shira al Yemeniat * ?	α Canis Majoris	187 40	15 43	S	4
65	Aoul al Adzari * *Prima Virginum*	ε Canis Majoris	191 0	29 0	S	4
66	Thanih al Adzari *Secunda Virginum*	o^2 Canis Majoris	193 0	24 5	S	2
67	Thalath al Adzari * *Tertia Virginum*	δ Canis Majoris	194 0	28 31	...	2
68	Mirzem al Ghomeisa ?	β Canis Minoris	196 40	9 0	N	4
69	Aoul al Dzira * *Prima Brachii*	α Geminorum	197 43	?	N	1
70	Al Shira al Shamiyat ?	α Canis Minoris	200 0	6 20	N	1
71	Muekher al Dzira *Posterior Brachii*	β Geminorum	201 0	38 46	N	1
72	Lisan al Shudja * *Lingua Hydri*	δ Hydræ	212 0	6 0	N	4
73	Al Nethra * ?	ε Cancri	215 0	20 49	N	Neb.
74	? Al Shudja * ? *Hydri*	ζ Hydræ	219 0	7 0	N	4
75	Shimalih al Teref * *Borealior Oculorum*	λ Leonis	224 0	25 0	N	4
76	Soheil al Fard *Soheil Solitarius*	α Hydræ	227 0	5 5	S	2
77	Djenubi Ras al Asad *Australior Capitis Leonis*	ε Leonis	235 0	25 50	N	2
78	Djenubi Djebha al Asad * *Australior Frontis Leonis*	η Leonis	250 0	19 0	N	2
79	Kalb al Asad * *Cor Leonis*	α Leonis	237 0	3 0	N	1

No.	Arabic Name.	Modern Name.	Right Ascension.	Declination.	Region.	Mag
80	Menkib al Asad *Humerus Leonis*	γ Leonis	240 0	21 50	N	...
81	Aoul al Batina * *Prima Crateris*	α Crateris	250 0	15 13	S	2
82	Djenubi al Cheratan * *Australior* ?	θ Leonis	254 0	17 33	N	3
83	Al Serpha *Vicissitudo*	β Leonis	262 0	17 0	N	2
84	Minkar al Ghyrab * *Rostrum Corvi*	α Corvi	266 0	22 0	S	3
85	Djenah al Ghyrab al Eymen *Dextra ala Corvi*	γ Corvi	269 0	15 0	S	3
86	Thanih al Aoua * *Secunda Latratoris*	η Virginis	270 0	0 36	N	2
87	Laouiyet al Aoua *Angulus Latratoris*	γ Virginis	276 0	1 13	N	3
88	Rijil al Aoua * *Pes Latratoris*	μ Virginis	277 0	21 0	S	3
89	Al Simak al Azal * *Al Simak Inermis*	α Virginis	287 0	8 0	...	2
90	Ramih al Ramih *Lancea Lanceatoris*	η Boötis	294 0	21 0	N	4
91	Wasat al Ghafr * *Media* ?	κ Virginis	298 0	8 0	S	4
92	Al Simak al Ramih *Al Simak Lanceator*	α Boötis	300 0	22 0	N	1
93	Menkib al Aoua al Aisr * *Humerus Sinister Latratoris*	γ Boötis	304 55	13 25	N	2
94	Mintek al Aoua *Cingulum Latratoris*	ϵ Boötis	307 5	...	N	2
95	Anwar al Ferkadain* *Fulgens Vitularum*	β Ursæ Minoris	307 0	76 20	N	2
96	Nir al Feccah * *Lucida* ?	α Coronæ	321 20	30 0	N	3
97	Al Niyat al Aoul * *Prima του al Niyat*	σ Scorpii	330 0	24 0	S	2
98	Kalb al Akrab * *Cor Scorpionis*	α Scorpii	332 0	27 38	S	1
99	Al Niyat al Thanih * *Secunda τοῦ al Niyat*	τ Scorpii	333 0	25 0	S	3
100	Al Sabyk al Aoul * ?	ζ Ophiuchi	334 0	...	S	...
101	Al Sabyk al Thanih * ?	η Ophiuchi	342 0	...	N	3
102	Menkib al Hadi * *Humerus* ?	...	343 0	11 0	N	3

No.	Arabic Name.	Modern Name.	Right Ascension.	Declination.	Region.	Mag.
103	Ras al Djathi * *Caput Ingeniculi*	α Herculis	344 56	44 0	N	2
104	Menkib al Djathi al Aisr * *Humerus Sinister Ingeniculi*	δ Herculis	345 0	14 0	N	...
105	Aoul al Shaoulat * *Prima Caudæ (Scorpionis)*	υ Scorpii	346 5	36 50	S	3
106	Thanih al Shaoulat * *Secunda Caudæ (Scorpionis)*	λ Scorpii	349 0	30 0	S	...
107	Fekhyz al Djathih al Aisr * *Femur Sinistrum Ingeniculi*	ι Herculis	350 0	13 20	N	3
108	Mirfek al Djathih al Aisr *Cubitum Sinistrum Ingeniculi*	μ Herculis	351 0	28 0	...	2
109	Rekbet al Djathih al Aisr * *Genu Sinistrum Ingeniculi*	θ Herculis	355 0	31 0	...	2
110	Aoul al Waridah *Prima τοῦ al Waridah*	γ Sagittarii	357 0	30 0	S	2
111	Thanih al Waridah *Secunda τοῦ al Waridah*	δ Sagittarii	358 0	30 0	...	4
112	Thalath al Waridah * *Tertia τοῦ al Waridah*	ε Sagittarii	359 0	30 0	S	3

Errors in R.A. of Al Achsasi and Al Tizini (reduced) for 1650.

	Al Achsasi.	Al Tizini.		Al Achsasi.	Al Tizini.
	° ′	° ′		° ′	° ′
α Arietis	−0 53	+0 30	α Virginis	+0 18	+0 12
α Ceti	+0 26	+0 17	α Boötis	+0 5	+0 13
γ Tauri	+0 2	+0 37	α Scorpii	+0 0	+0 27
α Aurigæ	−0 44	+0 44	α Herculis	+0 16	+0 14
β Orionis	−0 26	+0 28	α Lyræ	+0 37	+0 3
β Tauri	−0 13	+0 38	γ Aquilæ	+0 8	−0 7
α Geminorum	−0 19	+0 34	α Aquilæ	+0 21	−0 7
α Canis Minoris	−0 13	+0 6	α Capricorni	+0 55	+0 24
β Geminorum	−0 56	−0 33	α Piscis Australis	+0 28	+0 38
α Hydræ	−0 35	+0 3	α Pegasi	+0 46	+0 30
α Leonis	−0 24	−0 4	δ Pegasi	+0 24	+0 40
β Leonis	−0 47	−0 10	γ Pegasi	+0 31	+0 37

Notes.

1. Al Waridah = γ, δ, ε, and η Sagittarii, forming a trapezium, and signifies (the ostrich) going to drink at the river, *i.e.*, the Milky Way.

3. Al Sadirah = σ, φ, τ, and ζ Sagittarii, forming another trapezium, and signifies (the ostrich) returning from drinking.

4. Written Al Nesl for Al Nesr.

6. Not in Al Tizini.

8. The name Al Beldat, meaning "The Town," is given to a space in Sagittarius void of conspicuous stars. The Lucida of Al Beldat is therefore the brightest star nearest to this space.

13. The Declination is wrong.

14. Aquila is named in this MS. Al Nesr, the Vulture; Al Ghyrab, the Crow; and Al Akab, the Eagle.

16. Al Dzabihh, the Sacrificer = α and β Capricorni.

17. The Declination should be 39°. The scribe has been very careless in writing the Keff ك = 20. In some cases he has first written a Lam ل = 30, which he has subsequently altered by a diagonal stroke to a ك, and sometimes omitted doing so, which explains the errors in the Declinations of 71 and 84.

19. Cf. Al Sufi (Schjellerup), p. 189, as to the explanation of the name Saad Bula = Fl. 7, μ and ϵ Aquarii.

20. Saad al Saaoud = β and ξ Aquarii according to Ulugh Beg, but Al Sufi and Ideler give in addition ϵ Capricorni. The Declination is wrong.

23. Al Achbiya = γ, ζ, π, and η Aquarii.

26. Called by Al Tizini Nir Saad al Naam. Al Sufi gives Saad al Naam as τ and υ Pegasi, with neither of which does Al Tizini's position accord. Al Achsasi calls it Saad al Bahaim, which name is given by Al Sufi to θ and ν Pegasi. The position in both Al Tizini and Al Achsasi is that of ζ Pegasi.

28 and 29. Al Mukdim signifies "anterior," and in the description of these stars the words Al Ferg should precede. Al Ferg signifies the place between the handles of a bucket over which the water is poured.

31. This star is written in the MS. "Keff al Salsalat." In Al Tizini and Al Sufi it is called "Al Musalsalat." The Declination is quite wrong for ι Andromedæ.

33 and 34. Al Muchir signifies "posterior," and in the description of these stars the words "Al Ferg" should precede.

35 and 39. Scribe has written Declination 13 يج for 18 يح.

36. Written الشرشا, probably in error for الرشا.

37. Al Naamet, the Ostriches = τ, υ, ζ, θ, and η Ceti.

40. The star Al Djedi is out of its place, R.A. should be 95° 17′.

43. Al Sheratein, the Marks = β and γ Arietis.

44. Schjellerup translates Al Natih as "celui qui frappe de la corne."

46. Al Botein = δ, ϵ, and ρ Arietis. Dorn gives Nir Al Botein as δ Arietis, but the positions in Al Tizini and Al Achsasi do not agree with any of the above stars.

47. Declination 7° 11 for 2° 11.

51. It is commonly understood that the name Al Debaran belongs solely to the star α Tauri, and in Al Sufi it is restricted to that star; but it is certain that by many Oriental astronomers it was applied equally to the five stars α, θ, γ, δ, and ϵ Tauri, which form the Hyades. In this MS., No. 51, γ Tauri is called "The first star of Al Debaran," and No. 52, α Tauri, is "The bright star of Al Debaran." In the Alfonsine Tables, printed in 1483, γ Tauri, which is the eleventh star of Ptolemy's *Catalogue of Taurus*, is described thus: "Que sūt (sic) supra narē earū q̄ sūt ī facie et sūt stelle Aldebram"; and the name Al Debaran is not given at all to the fourteenth

star = α Tauri. The same words occur in a more correct form in the *Liechtenstein Almagest* (1515): "Que est super narem earum que sunt in facie: et sunt stelle Aldebarā," showing that Gerard of Cremona, whose translation this is, employed in the twelfth century a similar Arabic text to that used by Alfonsus in 1252.* But the description of α Tauri contains these words (writing the Latin *in extenso*): "Lucida, quæ . . . est ex forma Aldebaran quinta." Liechtenstein states definitely that the bright star α Tauri "is the fifth star of the figure Aldebaran." In Hyde's translation of Ulugh Beg the name Al Debaran is written against the above five stars, a point which Baily omitted in his edition.

Clearly, therefore, the name Al Debaran was used at Cairo and Samarkand, and also by Occidental Arabs, to designate not one star, but the five principal stars of the Hyades.

52. Declination is wrong.

55. Al Aiouk. No satisfactory explanation of the meaning of this name has hitherto been found.

56. The Declination is wrong.

61. Certain stars are designated "Al Yemeniat" clearly from the adjectival meaning of the word "the right" or "the right hand." But in the case of 64 α Canis Majoris, Al Sufi explains that the word signifies that the star *sets* in the direction of Yemen. Similarly in the case of 70 α Canis Minoris, he states that this star is called "Al Shamiyat," because it *sets* in the direction of Syria. It would seem that there is a mistake here, and that for "sets" we should read "rises."

62. Al Sufi says that a star which precedes a brilliant star is termed Al Mirzem. Scribe has written R.A. 173° for 183°.

63. Al Henat signifies "a mark burnt on the right side of a camel's neck." It is not quite certain which stars formed Al Henat, but according to Al Sufi they were either γ and ξ, or ν and γ Geminorum.

62 to 64. The scribe has written ج for ف in the Right Ascensions.

65. Al Adzari, "the Virgins" = o^2, δ, ε, and η Canis Majoris.

67. The Declination is that of η Canis Majoris. This star is not in Al Tizini.

69. Al Dzira, "the arm," "the foreleg" = α and β Geminorum.

72. This name is not in Al Tizini.

73 Al Nethra signifies the place under the nose between the moustachios, *i.e.*, of the Lion, the Arabian constellation of the Lion being in Cancer.

74. The first word is undecipherable.

75. Al Teref or Al Tarf = λ Leonis and κ Cancri. The R.A. is 4° in error.

78. The R.A. is clearly written 250°, but should be 237°.

79. Declination 3° for 13°.

81. The MS. has الباطنه instead of الباطيه. The Declination gives 35° for 15°; a common mistake in Arabic MSS. This star is not in Al Tizini.

82. The MS has الخراسان for الخرتان.

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* This is most probable, seeing that there is evidence from two MSS. at Merton College, Oxford, that Gerard of Cremona made some of his translations, from Arabic into Latin, at Toledo, where the Alfonsine Tables were constructed in the following century.

86. Al Aoua, the howling dog = β, η, γ, δ, and ϵ Virginis.

88. Not in Al Tizini. The scribe has written R.A. 277° probably for 287°

89. Al Simak. The meaning of this word is obscure. *Cf.* Ideler and Schjellerup.

91. In the Declination the scribe has written ع for ح.

93. Error in Declination of 13° يج for 38° لح

95. R.A. and Declination both wrong. The correct Declination is written against 94. Al Ferkadain, the Calves = β and γ Ursæ Minoris.

96. The meaning of the name Al Fecca is quite obscure. Declination is erroneous.

97. Al Niyat means "the vein which suspends the heart," and applies to σ and τ Scorpii, situated on each side of the heart of the Scorpion.

98 and 99. The Declinations should be interchanged.

100 and 101. Al Sabyk. Lane gives the meaning as "preceding," and says it is applied to stars which precede others. The Declinations of both stars are wrong for ζ and η Ophiuchi.

102. The name is clearly written, الجادي, Al Hadi, which is probably a mistake for الجاثي. The R.A. and Declination do not accord with any star in Hercules or Ophiuchus.

101 to 104. The Declinations of these stars are all wrong, and they suggest that the scribe has taken from Al Tizini by mistake the Declinations of 276, 277, 278, and 280, instead of 280, 281, 282 and 283. A comparison of the two catalogues here offers from these errors the strongest evidence of the source whence all the positions were obtained. The Declinations of 101 to 112 are mostly erroneous.

104. In the description الحاوي is written for الجاثي.

105. Al Shaoulat, "the upraised tail of the Scorpion" = υ and λ Scorpii.

107 to 109. In the description الجاني is written for الجاثي.

Note on the Proper Motion of Arcturus. By W. T. Lynn, B.A.

The large southerly proper motion of *Arcturus* was noticed by Halley so long ago as 1718, and twenty years afterwards Cassini II. estimated that its latitude had changed by about five minutes between the time of Tycho Brahé and his own, a century and a half later. Hornsby has an elaborate paper (read 1772 December 24) on it in vol. lxiii. of the *Philosophical Transactions* (p. 93), in which he determined the star's southerly proper motion to amount to $2' 36''{\cdot}81$ in seventy-eight years, or about $2''{\cdot}01$ annually. This value is remarkably near the truth.

As it seems to me that the value now usually adopted is slightly too small, I have made a new determination from com-

paratively recent Greenwich observations, which forms the subject of this note. In the first place I have compared the R.A. and N.P.D. in Pond's Catalogue of 1112 stars for 1830 with the Ten-year Catalogue for 1880, and secondly I have compared together the places given in the Nine-year and Ten-year Catalogues respectively. The two results are very close, as might be expected from the large number of observations included in the catalogues; and they, therefore, probably represent the true amount of motion with great accuracy.

Pond's place for 1830 is R.A. $14^h\ 7^m\ 54^s{\cdot}63$, N.P.D. $69°\ 55'\ 42''{\cdot}70$. The precession in R.A. is $+2^s{\cdot}8132$, and the mean precession in N.P.D. for the interval between 1830 and 1880, allowing for the secular variation, is $+16''{\cdot}962$. Applying these for fifty years, we find the place reduced to 1880 is R.A. $14^h\ 10^m\ 15^s{\cdot}29$, N.P.D. $70°\ 9'\ 50''{\cdot}80$. The actual place for that year in the Ten-year Catalogue is R.A. $14^h\ 10^m\ 11^s{\cdot}286$, N.P.D. $70°\ 11'\ 32''{\cdot}09$. The differences are $-4^s{\cdot}004$ in R.A. for fifty years, or $-0^s{\cdot}080$ annually; and $+1'\ 41''{\cdot}29$ in N.P.D. for fifty years, or $+2''{\cdot}02$ annually.

The place in the Nine-year Catalogue for 1872 is R.A. $14^h\ 9^m\ 49^s{\cdot}390$, and N.P.D. $70°\ 9'\ 0''{\cdot}09$. The mean precession is in R.A. $+2^s{\cdot}8132$ and in N.P.D. $+16''{\cdot}9135$, making for eight years $+22^s{\cdot}505$ and $+2'\ 15''{\cdot}31$ respectively. Applying these, the place reduced to 1880 is R.A. $14^h\ 10^m\ 11^s{\cdot}895$ and N.P.D. $70°\ 11'\ 15''{\cdot}40$. Comparing these with the place in the Ten-year Catalogue, given above, we find the annual proper motion of the star to be $-0^s{\cdot}0761$ in R.A. and $+2''{\cdot}086$ in N.P.D. It is evident that the actual proper motion in N.P.D. exceeds two seconds, though that used in the last Greenwich Catalogue is $+1''{\cdot}977$ (slighly larger than the B.A.C. value), which seems to be about $0''{\cdot}1$ too small. The proper motion in R.A. seems to be a little less than $-0^s{\cdot}08$, which the value assigned in the Ten-year Catalogue ($-0^s{\cdot}0799$) approaches very closely indeed.

For a further check, I have compared the place in the Greenwich New Seven-Year (epoch 1864) with that in the Ten Year. In forming these, as well as that in the Nine-year Catalogue, the fractional parts of the proper motion were applied to the result of each observation. Whether Pond did so or not seems doubtful. The so-called annual precession for *Arcturus* in his catalogue is in N.P.D. what we should call annual variation, and includes the proper motion as determined by Baily; but whether the fractional parts were used in reducing the separate observations does not appear.

The place in the Greenwich New Seven-year Catalogue is R.A. $14^h\ 9^m\ 27^s{\cdot}52$, N.P.D. $70°\ 6'\ 29''{\cdot}32$; that in the Ten Year is R.A. $14^h\ 10^m\ 11^s{\cdot}286$, N.P.D. $70°\ 11'\ 32''{\cdot}09$. The mean annual precession is in the former element $+2^s{\cdot}8131$, and in the latter $+16''{\cdot}923$; for sixteen years, $+45^s{\cdot}010$ and $+4'\ 30''{\cdot}77$. Comparing, then, the First Seven Year place reduced to 1880 (R.A. $14^h\ 10^m\ 12^s{\cdot}53$, N.P.D. $70°\ 11'\ 0''{\cdot}09$)

with that in the Ten Year, we find the annual proper motion in R.A. to be $-0^{s}\cdot078$ and in N.P.D $+2''\cdot0000$. This value is probably very accurate. The number of N.P.D. observations in the New Seven-Year is 103, and in the Ten-Year 184.

Blackheath : 1895 *May* 22.

Note on the Binary ι Leonis. R.A. $11^{h}\ 18^{m}\ 27^{s}$, *Dec.* $11°\ 6'\cdot5$, 1895. ($3^{m}\cdot9$ *and* $7^{m}\cdot1$.) By Alice Everett, M.A.

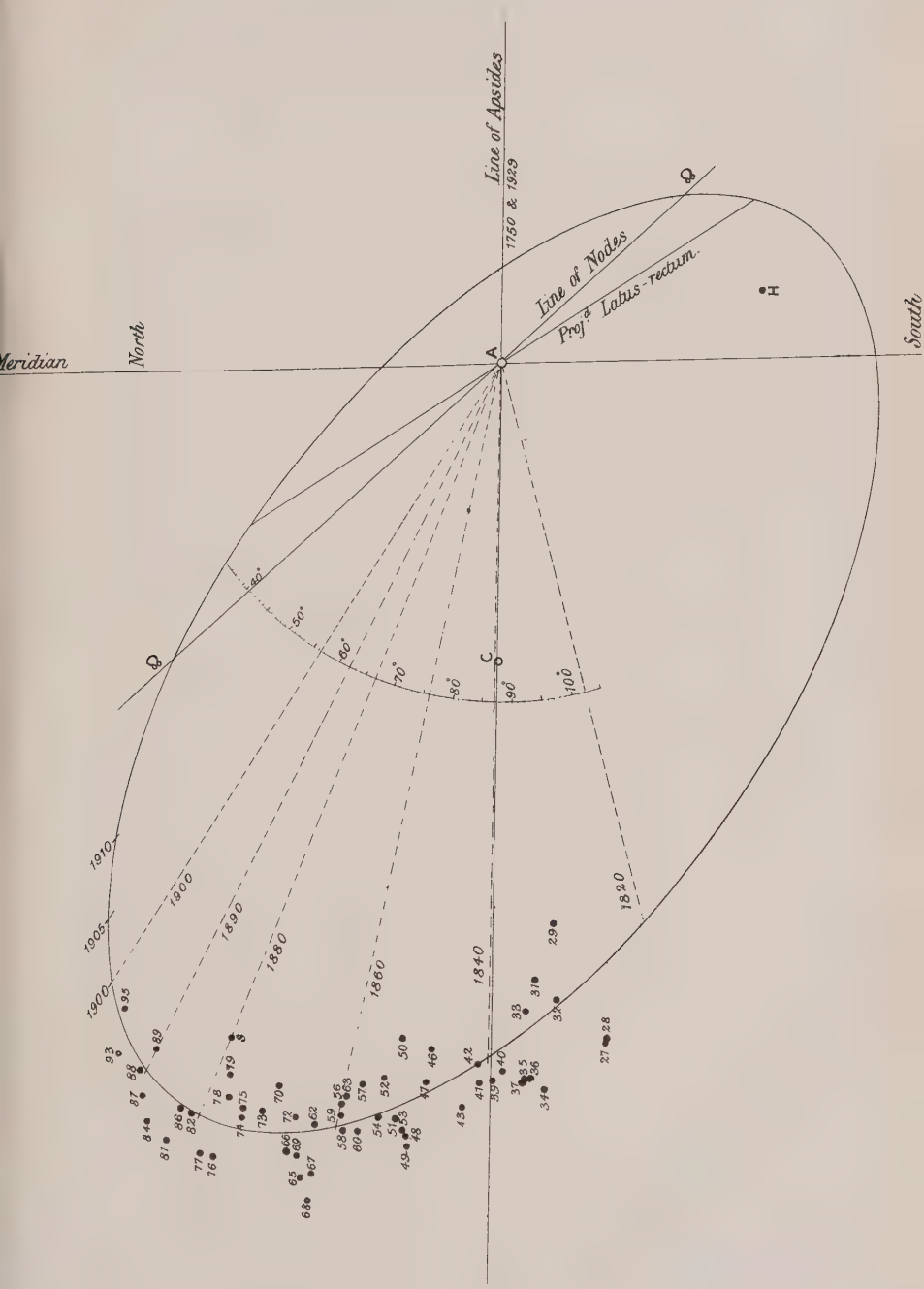
(*Communicated by T. Lewis.*)

Mr. Lewis, who had a suspicion that the companion star had passed the apse of its apparent orbit since Struve's time, suggested this binary among others to me for discussion. I have examined the measures, and, unless appearances are very misleading, think there is little doubt that the apse was passed recently, probably in the neighbourhood of 1880, and also, that maximum apparent distance was passed towards the end of the 'seventies, and that the distance is now diminishing comparatively rapidly. Another ten or fifteen years should settle the question, and as there seems to have been a tendency of late years to let the star drop out of observation, it is perhaps worth while to mention that there is at least a possibility that it is now at a rather critical part of its period. Measures were frequent from the time of Struve's first observation in 1827 until about 1883. I have not found any for the years 1885, 1890, 1891, 1892, and of those in the intervening years no two are by the same observer. This year's observation was made with the Greenwich 28-inch. The latest measures give—

1893·35	$\theta = 59°\cdot5$	$r = 2''\cdot625$
1895·29	$\theta = 58°\cdot2$	$r = 2''\cdot492$

The position angle has diminished about 40° since 1827. The period can scarcely be less than a century and a quarter ; I am inclined to think it is nearer two centuries, and it may be a good deal more. At the present stage the computation of a satisfactory orbit is out of the question, but I have made an attempt at a provisional orbit with the view chiefly of getting a trial ephemeris for the next few years. The results are—

P = 178·6 years	$\Omega = 40°\ 15'$
T = 1750·6	$\lambda = 249°\ 0'$
$e = 0\cdot7566$	$a = 2''\cdot486$
$\gamma = 65°\ 40'$	$\mu = -2°\cdot016$



Whittingwoode & C.o Lith. London.

The following are comparisons between observed and computed positions made at about five-yearly intervals. The mean of the observations in any year is taken :—

	θ			r		
	O.	C.	C−O.	O.	C.	C−O.
	°	°	°	″	″	″
1827·28	97·1	96·2	−0·9	2·30	2·09	−0·21
1829·37	93·9	94·5	+0·6	1·89	2·13	+ ·24
1835·33	90·4	90·4	0·0	2·40	2·24	− ·16
1836·40	90·6	89·7	−0·9	2·43	2·27	− ·16
1841·31	86·8	86·5	+0·3	2·40	2·35	− ·05
1846·31	82·8	83·5	+0·7	2·31	2·43	+ ·12
1851·42	80·4	80·7	+0·3	2·54	2·50	− ·04
1856·32	76·3	78·0	+1·7	2·53	2·56	+ ·03
1860·33	77·8	76·0	−1·8	2·60	2·61	+ ·01
1865·43	74·4	73·4	−1·0	2·79	2·65	− ·14
1866·30	73·5	73·0	−0·5	2·73	2·66	− ·07
1870·29	71·4	71·1	−0·3	2·52	2·68	+ ·16
1875·28	69·4	68·7	−0·7	2·63	2·70	+ ·07
1876·33	68·5	68·5	0·0	2·82	2·70	− ·12
1879·36	67·8	66·8	−1·0	2·54	2·70	+ ·16
1881·35	65·1	65·8	+0·7	2·82	2·69	− ·13
1884·53	63·3	64·3	+1·0	2·78	2·68	− ·10
1886·37	65·1	63·5	−1·6	2·69	2·66	− ·03
1889·16	61·9	62·1	+0·2	2·56	2·63	+ ·07
1893·35	59·5	60·0	+0·5	2·62	2·58	− ·04
1895·29	58·2	58·9	+0·7	2·49	2·53	+ ·04
			Ephemeris.			
1900·0	...	56·3	...	...	2·43	
1905·0	...	53·3	...	...	2·28	
1910·0	...	49·8	...	...	2·07	

Greenwich : 1895 *June* 14.

On the Variable Nebulæ of Hind (N.G.C. 1555) and Struve (N.G.C. 1554) in Taurus, and on the Nebulous Condition of the Variable Star T Tauri. By E. E. Barnard, M.A., D.Sc.

The subject of the variability of the light of a nebula is of the highest importance. There are many of these objects that have been reported variable; but so differently do many of the nebulæ appear in different telescopes and with different magnifying powers, that no sort of confidence can be placed in most of these statements, and a positive proof of an actual variable nebula, except in one case, is sadly wanting. It is quite reasonable to believe that some of the nebulæ do vary in their light, but, from the peculiarities above mentioned, it will take decidedly strong proof to convince astronomers who are familiar with these objects that some deception has not misled the observer.

A better example of this kind of error is scarcely wanting than the celebrated *Merope* nebula of the *Pleiades*, which for so many years was believed by half the astronomers to be variable, while the other half refused to believe that it existed at all, but I think, since it has been so easily and repeatedly photographed, that few will call it variable now, and no one will doubt its existence.

This, however, is a class of nebula that needs, from its diffused nature, a low power and large field, for contrast, more than anything else, to make it visible. It is therefore particularly suited for small telescopes.

The more definite and condensed nebulæ are not so much subject to deceptions of this kind, due to telescope and magnifying power, and estimates of their brightness by different observers with different instruments in many cases do not vary greatly, so that any very great discrepancy in descriptions by careful observers might be suggestive of actual variability. Such nebulæ are better seen in the more powerful telescopes.

To this latter class we must refer Hind's variable nebula in *Taurus*. This object was discovered by Mr. J. R. Hind, 1852 October 11, at Mr. Bishop's Observatory, London.

It remained visible for a few years, with ordinary telescopes, and then seems to have faded from view.

In *Astronomische Nachrichten*, No. 839, we find Mr. Hind's original account of the discovery of this object. He says: "Last night (October 11) I noticed a very small nebulous-looking object in A.R. $4^h\ 11^m\ 50^s$ $\delta = +19°\ 8'$ for 1825·0, the epoch of our ecliptic charts; it was south, preceding a star of 10 mag., which, to my surprise, has escaped insertion on the map for 4^h R.A. recently published—possibly it may be variable. [This is the variable star T *Tauri*.] The sky at the time was remarkably clear, but the object appeared faint; it preceded $1^s\cdot2$, and was $0'\cdot7$ south of it [of T *Tauri*]. I suppose it will

prove a new nebula, none of our catalogues having anything in the above position. Its diameter did not exceed 30″."

This was written 1852 October 12. This nebula was subsequently observed by D'Arrest, Struve, Lassell, and others. D'Arrest measured its position on four occasions during 1855 and 1856, but on 1861 October 3 it had disappeared. On that date he makes the following note: "I am not able to find any trace of this nebula, first seen by Hind in 1852. It was observed the first time by me, and frequently, during the years 1855 and 1856. Its position being determined four times. . . . Is not this nebula variable?"

He sought for it again 1861 October 4: "Sky very clear. I have examined the region with extreme care, aided by Schjellerup; the nebula is certainly absent."

Again he says: "I have searched for this nebula, with curiosity, under varied conditions during the months of 1862 January and February, but have found no certain evidences of it. That it has vanished meanwhile from the sky is attested by many astronomers, with more powerful telescopes than mine."

He states, however, that O. Struve and Lassell, with their great telescopes, never entirely lost this object during 1861 to 1864, but that by 1868 it had also absolutely vanished from the Pulkowa telescope.

When examining the region of this object early in 1868 Struve found another small nebula 4′ preceding the place of Hind's nebula. This was subsequently observed by D'Arrest, who was sure that no nebula previously existed at that place.

Both these nebulæ were observed by D'Arrest on three or four occasions, but, of course, not on the same dates. From these positions he gives the places of these objects for 1868·0:

	h m s	° ′ ″
Struve's Nebula	α 4 14 0·4	δ +19 12 13
Hind's Nebula	4 14 15·5	+19 12 27

He says (*A. N.* 1689), "Struve's nebula is pretty small, nearly round, with eccentric nucleus = × 14 mag. In a full dark sky it would be equal to Herschel's Class II. Hind's variable nebula in 1856 was larger and considerably brighter [than Struve's in 1868]."

He would place Hind's object as faint in Herschel's Class I.

In the above paper D'Arrest gives the place of T *Tauri* 1868·0.

$4^h\ 14^m\ 17^s{\cdot}8 \qquad +19^\circ\ 13'\ 4''.$

In observing Hind's nebula, 1855 November 3, D'Arrest thus describes it: "Discovered by Hind, *Astr. Nachr.* No. 839. A very bright nebula, 1′ in diameter; on its northern edge is a 10^m star, which follows $2^s{\cdot}20$, and is about 35″ north of the middle of the nebula."

In *A. N.* 2212 Tempel has a very important and interesting

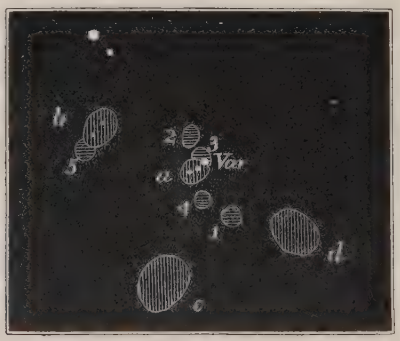
account of his observations of this region. They were made in 1877, and all the dates below refer to that year.

On November 5 he could see no nebulosity near T *Tauri* in the place assigned to Hind's nebula. On November 8 he found a nebula $1\frac{1}{2}'$ diameter, in the northern part of which a small star shone. This nebula was 1′ south and 15^s preceding T *Tauri*, and was recognised by him as the Struve nebula.

On December 12 he saw, in a very clear sky, two very small stars very close to T *Tauri*, but could see no nebulous appearance as he had so often seen previously. He could not see anything of the Struve nebula which he had seen November 8, but he saw two small stars at its place, the northern of which was the one he had seen on November 8 in the nebula, and which was about 40″ south following.

Tempel gives a very interesting sketch of the region in the paper referred to. This I have thought of sufficient interest to be here reproduced, so I send a copy of it. This is valuable, as it seems to point to the existence of several variable stars in this remarkable region.

Tempel found that the different positions of Hind's nebula as previously reported did not very well agree, so he has charted each of these for comparison.



1 = Hind's position as given by him in *A.N.* 839.

2 = The same, from Hind's description.

3 = Auwer's position. Herschel's Gen. Catalogue.

4 = D'Arrest's position in *Sid. Nebulosorum.*

5 = Struve-D'Arrest's position in Dreyer's supplement to Gen. Cat. = the place of Struve's nebula.

a = The nebulous appearance which Tempel had so often seen about T *Tauri*, and where later he saw only two small stars without any nebulosity.

b = The nebula which he (Tempel) had seen 1877 November 8, and which he recognised as Struve's nebula. The southerly of the two small stars at this point is the one described by D'Arrest as being excentrically placed in the Struve nebula.

c and *d* are small star clusters seen by Tempel. The scale of this sketch is 1 millimeter = 1^s.

Dr. Dreyer, in his Supplement to Herschel's Gen. Cat. of Nebulæ, says in 1877 : "No. 1689 of the *Astr. Nachr.* contains an observation by D'Arrest of an S nebula with an excentric nucleus

= ✲ 14m, which was first seen by O. Struve early in 1868, 15s p. the place of the missing [Hind's] nebula. At present there is no nebulosity distinctly visible, neither around this faint star, nor near the well-known variable star. On this point I am in perfect accordance with Dr. Copeland, observing with the large Dunecht refractor, and M. Tempel, who works with a fine Amici refractor of eleven-inch aperture (at Arcetri). In the Pulkova refractor, however, some traces of nebulosity seem still to be visible. M. Otto Struve informs me that he, from time to time, has observed the variable nebula, but that he avoids reducing and comparing his observations for fear of being pre-occupied with respect to this minimum visible. He does not consider the *Nova* from 1868 a separate nebula. 'What I see is certainly the variable nebula itself, only in altered brightness and spread over a larger space. Some traces of nebulosity are still to be seen exactly on the spot where Hind and D'Arrest placed the variable nebula.' "

After this these objects seem to have been fairly neglected by observers until 1890, when Mr. Burnham took up the matter with our great refractor. His paper on the subject was printed in *Monthly Notices* for 1890 December. As he states, he examined this region only under the best atmospheric conditions.

Misled by the position of T *Tauri* = *D.M.* + 19°·706 given in *D.M.*, he erroneously assumed that Hind's nebula was identical with the variable star. He says: "The place of the nebula as given by Dreyer, on the authority of D'Arrest, is identical with that of *D.M.* + 19°·706, the magnitude of which was estimated by Argelander as 9·4; and this is T *Tauri* of the variable star catalogues." This statement is true, but the position of *D.M.* + 19°·706 as given in *D.M.* is erroneous.

The *D.M.* place of this star and the position of Hind's nebula, reduced to 1855·0, are as follows:

D.M. + 19° 706	α = 4h 13m 31s·2	+ 19° 10′·6
Hind's Nebula	= 4 13 31	+ 19 10·5

A sufficiently close agreement to suggest identity, which was further heightened by the fact that Mr. Burnham found that T *Tauri was* a nebula!

But, singularly enough, the *D.M.* place of T *Tauri* is about 1s·8 too small in *A.R.*, and 0′·7 too small in δ, almost exactly the difference in right ascension and declination between the true place of Hind's nebula and T *Tauri*. So much so, indeed, that the *D.M.* place of T *Tauri* actually agrees with the position of Hind's nebula as given by D'Arrest!

Chandler, in his Variable Star Catalogue, *A.J.* 179, gives the place of T *Tauri* for 1855·0

4h 13m 33s	+ 19° 11′·3

and this is the true place.

On the assumption, therefore, that T *Tauri* was Hind's nebula, Mr. Burnham says, in his paper : "This small star, if it is a star, is placed within a very small condensed nebula. It is somewhat elongated in the direction 151°·7. A rough reading of the wires gave 4″·4 for the length of the nebula in this direction. It is less extended on the opposite side of the star or nucleus, with a shorter diameter of perhaps half that measured. It was examined with various powers, but it was impossible to say whether or not it had a disc like that of an ordinary star. If it were the bright nucleus of the small nebula, it would probably present the same stellar appearance. It will be noticed that the description of the nebula does not correspond with that of the early observations, where it was noted, when it was seen at all, as about 1′ in diameter."

Mr. Burnham asked me to examine this object with him, and I well remember that the small star was placed in an elongated nebula just as he describes.

The nebula was conspicuous and definite, not a nebulous glow. One would not hesitate a moment as to its being a very small fairly bright definite elongated nebula, the star to all appearance simply being a nucleus to the nebula.

In examining this region with the 36-inch at Mr. Burnham's request with him, I saw a very faint nebula close south preceding T *Tauri*. Of this Mr. Burnham says : "On [1890] October 15 I asked Mr. Barnard to examine this region with the large telescope. . . . After careful attention he was able to see an excessively faint round nebula, about $\frac{3}{4}'$ from the one previously described [T *Tauri*], in the estimated direction of 185°. This faint nebulosity was about 40″ or 50″ in diameter, and apparently not connected with the variable, and was of nearly the last degree of faintness for the light-power of the large instrument. It is perhaps too faint for any other telescope. Neither Mr. Barnard nor myself on any occasion could see the slightest trace of the OΣ nebula (seen also at times by D'Arrest, Tempel, and others, but invisible in the Rosse reflector), which should be 15ˢ preceding Hind's, nor any nebula in the immediate vicinity. Subsequently we looked at the small nebula [T *Tauri*] with the 12-inch telescope, by way of getting a better estimate of its magnitude, and found that it was a very faint object, and in appearance precisely like any ordinary star of that magnitude. The nebulous surrounding was completely lost with the smaller instrument. I do not think it can be any brighter now than 12·5 magnitude of the scale used in my double star observations."

As a proof of the nebulous character of T *Tauri*, I quote from Mr. Burnham's paper further : "On the night of November 1 Mr. Keeler examined the nebula [T *Tauri*] with the small spectroscope, and found that it was probably of the usual gaseous type, although on account of the extreme faintness of the nebula only the principal line at λ 5005 was visible. The spectrum of the nucleus could not be seen."

It will be noticed here that though Mr. Burnham was erroneously led, by the error in the *D.M.*, to identify Hind's nebula with T *Tauri*, he discovered the important fact, which was previously unknown, that the variable star T *Tauri* is a nebula—or a nebulous star.

I have quoted thus extensively from Mr. Burnham's paper, because I have recently made some observations of these objects that bear directly upon his work, as will be seen.

During his examination of this region Mr. Burnham—whose excellent custom it is to measure everything he comes across that is measurable, and who does not leave on record a simple statement only of what he has seen—measured the position of T *Tauri* with reference to the brighter star *D.M.* + 19°·704 (which is *W.B.* 4ʰ·274). He first measured the difference of declination, and got

1890·775	Δδ	243″·65
·777		243·40

He then measured the position angle and distance,

			Mags.	
1890·775	43°·5	336″·31	8·3	11·7
·777	43·8	336·07	8·4	12·5

From these last I derive the following differences (T *Tauri*—*W.B.* 4ʰ·274):

1890·776 Δδ = +0ᵐ 16ˢ·39 Δδ = +243″·25.

Having assumed, with Mr. Burnham, that Hind's nebula was really the nebula T *Tauri*, I paid no further attention to the subject until the past winter, when it occurred to me to look up the faint nebula I had seen close s.p. T *Tauri*.

On 1895 February 25 I examined the place with the 36-inch and found the nebula very easily; it was faint, but not extremely so. Indeed it was by first finding the nebula that I identified T *Tauri*, so different did that star look from my previous inspection of it in 1890 with Mr. Burnham.

The star was bright, not less than the 10th magnitude, and the small nebula in which it shone in 1890 had absolutely disappeared. With no power could I see the small elongated nebula of which the star had previously formed the nucleus. The additional brightness of T *Tauri* could have nothing to do with this if the nebula was as conspicuous as in 1890. The star seemed perfectly stellar and of a reddish-yellow colour.

By comparing it carefully with a star of similar colour and magnitude, 50·6 seconds preceding and 3′ north—which I shall hereafter refer to as A for convenience—I could, however, see that T *Tauri* was certainly surrounded by a very feeble indefinite nebulous glow for a few seconds of arc, but the definite nebula had certainly disappeared.

As a further identification I measured the position of the star with reference to the 8m *D.M.* star south preceding :

$$\Delta\delta + 243''.49 \text{ (2 obs.)}$$

which agrees with Mr. Burnham's $\Delta\delta$ of 1890.

A rough difference of right ascension gave 17^s, thus showing the identification was complete, and this has been further verified (see March 28, later).

As I have said, the nebula close south preceding the variable star was easily visible. It was round, quite definite, and very feebly brighter in the middle ; and was estimated to be of about the 16th magnitude. I think it was somewhat brighter than in 1890.

The following micrometrical position was obtained (nebula—T *Tauri*) :

$$\Delta\alpha = -0' 26''.05 = -0^m 1^s.85 \text{ (2 obs.)}$$
$$\Delta\delta = -0\ 26''.26 \text{ (3 obs.)}$$

It did not reach to the star, from which it seemed to be entirely independent.

I also carefully examined the place of the Struve nebula. There was no nebulosity visible at this point, close to which, however, was a small star of the 14-15th magnitude. I could not fully convince myself that there was not a feeble glow about this star, but it was too uncertain to be in any way positive.

Comparing my measures of the position of T *Tauri* with that given in the *D.M.*, I found, to my surprise, that the supposed coincidence of position with Hind's nebula was due to an error in the *D.M.* position of the star. I then looked up Hind's original account of his discovery in *A.N.* 839, and saw at once from his description that his nebula was by no means identical with T *Tauri*, and that he had observed both objects at that time, and furthermore that his nebula had the exact position occupied by this faint nebula close south preceding T *Tauri*, which I had just measured ; and that, indeed, this faint nebula, seen by Mr. Burnham and myself in 1890, was nothing else but Hind's nebula of 1852, which was supposed to have vanished from the face of the heavens !

On February 26 I examined this region with the 12-inch. T *Tauri* was easily visible. I could catch the feeblest glimpses of Hind's nebula by occulting the star ; if I had not known it was there, however, I could not have seen it, it was so excessively difficult.

I estimated T *Tauri* to be of the 10th magnitude. It was of equal brightness to the star A.

On March 4, in moonlight, with the 36-inch I measured the position of the small star close to the place of the Struve nebula, with reference to the *W.B.* star ($4^h.274$) south of it.

(Small star—*W.B.* $4^{h}\cdot274$)

$$\Delta\delta = +0'\ 8''\cdot29 \qquad = +0^{m}\ 0^{s}\cdot59 \text{ (3 obs.)}$$
$$\Delta\delta = +3\ 8\cdot51 \text{ (3 obs.)}$$

The resulting position shows this is the object mentioned by D'Arrest as being a 14-mag. nucleus excentrically placed in the Struve nebula.

On this occasion I noted: "T *Tauri* with 520 certainly surrounded with a dense hazy nebulosity several seconds in diameter. The image of the star carefully focussed, it was perfectly stellar. It was compared carefully with the star A. Both of the same colour and magnitude. A was free of the nebulous glow seen about T *Tauri*."

On March 24 I again examined the region with the 36-inch. The sky was poor. Only the vaguest traces of Hind's nebula could be seen on account of the poor sky. "Not certain, but there is a slight haziness at the place of the Struve nebula. With 350 and 520 diameters T *Tauri* seems to be a reddish star in a feeble glow of nebulosity. Compared with A, T *Tauri* is certainly feebly nebulous, but the star itself is stellar. The nebula that Mr. Burnham and I saw in 1890, in which this star was placed, certainly does not now exist. T *Tauri* is, I think, $\frac{1}{10}$ to $\frac{2}{10}$ magnitude fainter than A. I think it fainter than at previous observations. The two stars reddish and very much alike."

On March 28 with the 12-inch, from eighteen transits, I determined the $\Delta\alpha$ of T *Tauri* and the *W.B.* star $4^{h}\cdot274$—T *Tauri*

$$\Delta\alpha = +0^{m}\ 16^{s}\cdot44,$$

which differs only $0^{s}\cdot05$ from the value derived from Mr. Burnham's measures. At this observation T *Tauri* was of the same brightness as A.

If we refer to Tempel's sketch and descriptions we shall see that these observations have shown only one star at the place of the Struve nebula where he saw two. No star was seen near T *Tauri*, close north of which Tempel saw two that had finally taken the place of a nebula which had vanished. No trace of this nebula was seen.

Tempel's work seems to have been carefully done—his stars are missing. One can hardly doubt—knowing the remarkable nature of this region—but that these objects which he saw, and which are not now visible, are variable also, or of a temporary nature.

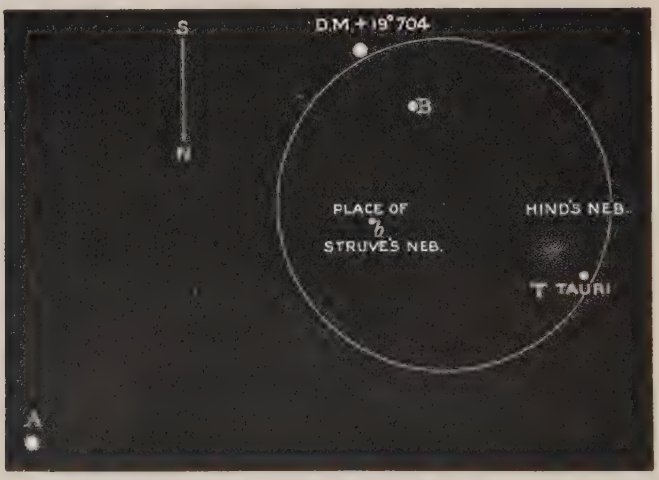
In reference to the actual nebulous condition of T *Tauri* there is no question but this star in 1890 October and November was the nucleus of a very small pretty bright nebula, as recorded by Mr. Burnham. There is also no question but that this small nebula had disappeared from the star in 1895 February and

March, and that the star was perfectly stellar, but involved in a very faint diffused nebulous glow.

At the time of Mr. Burnham's observations this variable star was at its minimum brightness, and at the present observations it was bright though not at its maximum. Is it possible that in its fainter phases this star becomes essentially a very small nebula, or the nucleus of such, and at its maximum the nebulosity is absorbed into the star, or otherwise disposed of? I hope to carefully watch this star hereafter, and try to settle some of these points.

I think it probable there is a slight fluctuation in the light of Hind's nebula, for it was, I am confident, somewhat brighter in 1895 February than in 1890 October.

I send a sketch showing the present appearance of Hind's nebula with respect to T *Tauri* and several other stars. It is less than $\frac{1}{2}'$ in diameter.



The star A is the one to which the light and physical appearance of T *Tauri* were referred.

B is a 12·5 or 13 mag. star, whose place on the sketch is by eye estimation.

b is the small star which was measured by me, and which was involved in the north part of Struve's nebula according to Tempel

There may have been other faint stars in the field, but there were none near *b* nor near T *Tauri*.

From my observations of the relative positions of T *Tauri* and *W.B.* $4^h{\cdot}274$, and adopting the position of the latter star as given in the catalogue, the following is the place of T *Tauri*:

1895·0 $4^h\ 15^m\ 52^s{\cdot}39$ $+19°\ 17'\ 8''{\cdot}1$.

The resulting place of Hind's nebula will be

1895·0 $4^h\ 15^m\ 50^s{\cdot}54$ $+19°\ 16'\ 42''{\cdot}1$.

Also, from the *W.B.* star, the following is the position of the 14th magnitude star supposed to be the one forming the "excentric nucleus" of Struve's nebula :—

1895·0 $\alpha = 4^h\ 15^m\ 36^s{\cdot}54$ $\delta = +19°\ 16'\ 13''{\cdot}1$.

These, carefully reduced to 1860·0, become

	h m s	° ′ ″
Star in Struve's Nebula	$\alpha = 4\ 13\ 34{\cdot}39$	$\delta = +19\ 11\ 1{\cdot}6$
Hind's Nebula ...	$\alpha = 4\ 13\ 48{\cdot}37$	$\delta = +19\ 11\ 31{\cdot}1$

These objects are respectively Nos. 1554 and 1555 of Dreyer's New General Catalogue of Nebulæ ; their places, as given in N.G.C., for 1860·0 are

	h m s	° ′
Struve's Nebula (1554)	$\alpha = 4\ 13\ 33$	$+19\ 11{\cdot}0$
Hind's Nebula (1555)	$\alpha = 4\ 13\ 48$	$+19\ 11{\cdot}2$

These two nebulæ, Hind's and Struve's, make the best authenticated case of variability in the light of a nebula that we have. The facts concerning them stand incontestable.

Here were two nebulæ which less than half a century ago were easily visible in ordinary telescopes, and which were bright enough to be placed, one in Herschel's first class, the other in his second class, and which were observed and measured by the best observers then living. One of these nebulæ has disappeared from the face of the heavens, the other has become so faint that it can be seen only in the most powerful telescope.

There is a satisfaction in this—it is so above any uncertainty, and the facts are so clear, that even the feeblest doubt can never be raised to question the case.

Another Probable Variable Nebula.

There is nearly as remarkable a case as the foregoing, which I have myself encountered. Unfortunately, however, it rests alone on my own observations, and therefore has not the independent testimony of other observers that should always be required to satisfy the scientific spirit of the age. The facts are none the less true, and may some time be supported by other evidence, and so I shall insert them here to await the testimony of time.

This was the case of a nebula which I found with the 12-inch 1888 November 30. I have already called attention to this object in *A.N.* 3097, but I wish further to place it on record in this paper, as it should properly be recorded in connection with the present subject. As the account in the *Nachrichten* is clear, I shall copy a portion of it for record here, and insist that

I am still as fully convinced as I was at the time I wrote the article, that the nebula became visible as a bright object where no nebula was previously known, and then, some time between 1888 November 31 and 1891 November 22, faded down to a very faint object in the same telescope in which it had appeared conspicuously bright.

Here is the account from *A.N.* 3097 : "On 1888 November 30 I discovered a small pretty bright nebula in *Cetus*, and was surprised, from the brightness of the nebula, to find that it was not in any catalogue. I carefully measured its position with the micrometer, and examined it the next night for motion, suspecting it to be a comet. No motion being detected, it was not observed further. I have a very distinct recollection of the object, and from my description I would estimate that it was between the 9th and 10th magnitudes. In its centre was a very small stellar nucleus of the 13th magnitude. Not having seen the nebula in my subsequent sweeps in that neighbourhood, I was led in 1891 to examine its position with the 12-inch. The nebula was found with some difficulty. It was extremely faint, and was only identified by the aid of the comparison star of the previous observation. This was 1891 November 22. I estimated it to be $13\frac{1}{2}$ magnitude, $\frac{1}{2}'$ in diameter, with perhaps a faint nucleus. I again examined it on 1891 December 24, and could see a very faint nucleus. The nebula was estimated 13 magnitude, round, $\frac{1}{2}'$ in diameter. In a note it is recorded that from recollection the nebula at these last observations could not be one-fifth as bright as on 1888 November 30.

"Following are the observations of the nebula 1888 November 30 (neb $-$ ★) :

$$\Delta\alpha = +1^m\ 44^s{\cdot}13 \text{ (10 obs.)}, \qquad \Delta\delta = -5'\ 47''{\cdot}2 \text{ (3 obs.)}.$$

The comparison star was W_1 $0^h{\cdot}594$, the position of which for 1888·0 was $\alpha = 0^h\ 36^m\ 11^s{\cdot}58$, $\delta = -8°\ 42'\ 19''{\cdot}3$, and the position of the nebula

$$1888{\cdot}0 \quad \alpha = 0^h\ 37^m\ 55^s{\cdot}71, \qquad \delta = -8°\ 48'\ 6''{\cdot}5.$$

"The observations of 1891 November 22 gave (neb $-$ ★)

$$\Delta\alpha = +1^m\ 43^s{\cdot}88 \text{ (6 obs.)}, \qquad \Delta\delta = -5'\ 49''{\cdot}0 \text{ (3 obs.)}."$$

I have examined this nebula several times since with the 12-inch, but it has always been a faint object.

Mount Hamilton, California :
1895 *April* 1.

On a Great Photographic Nebula in Scorpio, near Antares.

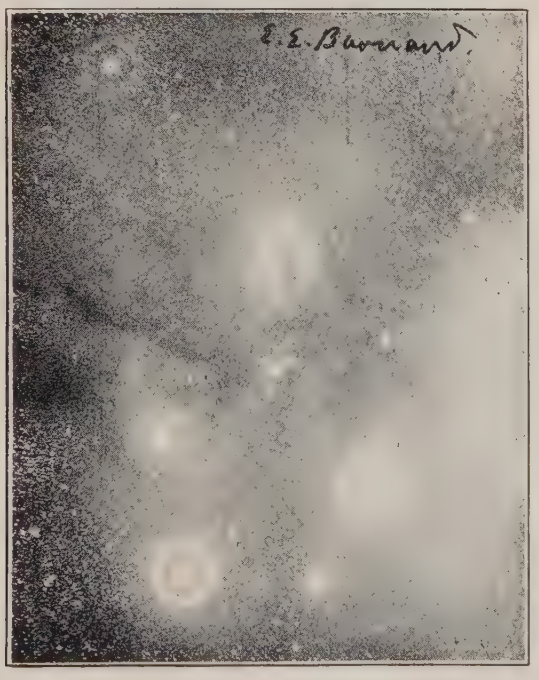
By E. E. Barnard, M.A., D.Sc.

For many years I have known of a vast but vague nebulosity in the region of *Antares*. This has been encountered in my comet seeking. It was so indefinite, however, that it could not be definitely located. With a large field, on a small telescope, the entire sky in that region seemed to be covered with a very feeble nebulosity. A portion of this I had located about 22 *Scorpii* and at other points.

I have recently photographed this region. The resulting pictures have shown that a magnificent nebula occupies a large portion of this region of the sky. The brightest portion of it condenses about the stars.

		m	h	m	s	°	′
ρ Ophiuchi, 22 *Scorpii*, and two small stars in (1875·0)	{	9·6	16	17	47	−24	9·3
	{	8·0	16	17	52	−24	10·3

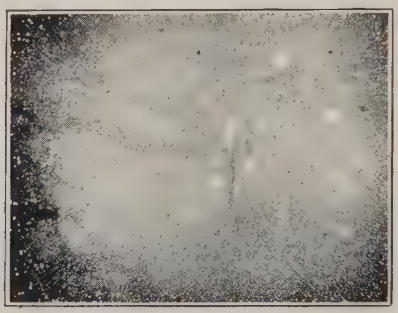
This great nebula occupies a remarkably vacant region. It seems to fill in a great hole among the stars. From this vacancy



Great Nebula near *Antares*, 1895 April 19. Lick Observatory, 6 in. Willard portrait lens.

there are several vacant lanes that run easterly from it for many degrees, and which are entirely devoid of stars. Even though there were no nebula here, the wonderful appearance of the sky itself would be sufficiently remarkable to attract attention at once.

This great nebula is very intricate and complicated in form. Especially is this shown where it condenses about the above-mentioned stars. It is directly connected with σ *Scorpii* by a stream of nebulosity that widens and intensifies where it involves σ. I have made four photographs of the nebula with the Willard lens, and three with the "lantern lens." Because of its great southern declination it is not possible to give as long an exposure as I should wish.



Great Nebula near *Antares*. 1895 March 30. Exposure 2h. 8m.
Lick Observatory, $1\frac{1}{2}$ in. lantern lens.

I send a glass positive of one of these negatives with the Willard lens on 1895 April 19. I would call attention to the fact that the stars on this picture are very imperfect. The low altitude of the object placed the telescope in an awkward and unusual position, which caused an unexpected strain to be brought to bear on the instrument. This, however, has but little effect on the nebula, except, perhaps, to slightly mar some of the more delicate details. I have been waiting for another opportunity to give a longer and better exposure, but bad weather has prevented.

I have selected one of the negatives made with the "lantern lens," and from this I also send a glass positive. This particular picture gives one an admirable idea of the marvellous nature of this part of the sky. It also shows the nebula and its singular relation to the vacancies of that region. The Willard lens had shown feebly that all the bright stars near the nebula were involved in it.

The small picture more clearly shows this. It will be seen that a large diffused portion of it extends all over the region about *Antares* and σ *Scorpii*, and several degrees beyond.

This picture also shows a large irregular long nebula involving the star *ν Scorpii*, which was not previously known.

In looking at the larger of the two photographs, it will be seen that *22 Scorpii* lies in a rift of the nebula, between two brightish strips which lie close north and south of it, and which are inclined to each other.

All the details shown are readily made out on all the different negatives—for both lenses.

The different pictures with the Willard lens show several other stars in this region to be nebulous. A list of these has been sent to the *Astronomische Nachrichten*.

The first of the negatives with the Willard lens (March 23), in which the stars are very perfect, shows M 4 to be a very beautiful star cluster, just sufficiently loose to show the individual stars nicely.

A word in reference to the "lantern lens." I have given an account of some interesting experiments with this lens in *Astronomy and Astro-physics* for December 1894.

This is a small magic lantern lens, from a cheap oil lantern. It is $1\frac{1}{2}$ inch in diameter, and 3 inches focus from the back lens, the equivalent focus being about 5 inches. By averaging the focus it is possible, as will be seen by the picture, to get a fairly flat field over some 20° or 25°. Its action is very rapid, and the prominent cloud forms of the Milky Way are shown in some ten minutes' time. For many purposes it will be extremely valuable, far more so than any larger or more powerful lens. It is so inexpensive, and its results are so wonderful, that every amateur, and every professional observatory, should possess one. I would specially call the attention of the Society to this kind of lens, and would not wish a better specimen to illustrate its capacity than the picture I send with this (made 1895 March 30). On some of the other negatives with this lens (of this region), familiar stars are shown by having the stars in the centre in exact focus, but this hurts the rest of the field, and impairs the usefulness of the lens. It is therefore well to average the focus with a lens of this kind, and thereby extend the flatness of the field.

I am very much pleased to see that Dr. Sheldon has taken up this subject with a similar lens since I called attention to the wonderful results of such. His article on this subject will be found in the *English Mechanic*, 1895 February 15.

Mount Hamilton, California:
1895 *May* 4.

On a Photographic Study of the Earth-lit Portion of the New Moon. By E. E. Barnard, M.A., D.Sc.

I have recently been making some experiments in photographing the Earth-lit portion of the new Moon with the 6-inch Willard portrait lens. The more prominent details are easily shown in the earlier stages, before the Moon becomes too old.

I have thought it would be interesting to make a series of such photographs, both in the evening and morning phases, for the study of any peculiarities in this illumination.

The first of these pictures was made on 1895 February 26, at about half-past seven. This is a very beautiful picture. The Earth-lit globe stands out beautifully round, encircled by the slender crescent. All the "seas" are conspicuously visible, as are also the other prominent features, especially the region about *Tycho*. *Aristarchus* and *Copernicus* appear as bright specks, and the light streams from *Tycho* are very distinct.

Photograph of New Moon (enlarged), 1895 February 26. 7^h or 8^h. Exposure 30^s. Lick Observatory, 6 in. Willard portrait lens.

For the inspection of the Society I send two glass positives from this picture; the smaller one is a direct contact positive, and is consequently of the original size, while the other is enlarged a couple of times. The exposure was 30 seconds.

Mount Hamilton, California:
1895 *May* 4.

The Thermal Radiation from Sun Spots. Observations made at Daramona, Streete, co. Westmeath. By W. E. Wilson.

In 1894 March Dr. Johnstone Stoney kindly communicated to the Royal Society some observations of mine on the above subject.* In this preliminary note a full description is given of the instruments and of the method of observations. It is therefore unnecessary to again describe them, but merely to mention that a large image of the Sun is formed by means of a heliostat and projected on to a screen in the observatory. Behind this screen, and standing on a pier of concrete, is placed a Boys radio-micrometer. A small pinhole in the screen admits radiant heat into the radio-micrometer. By placing any part of the solar image over the pinhole we can measure the relative radiation of this part. Two long rods and handles come into the observatory from the heliostat, and by turning these any desired part of the solar image can be accurately placed over the pinhole aperture. The driving clock of the heliostat then keeps it in this position while an observation is being made. The standard to which all the values of radiation from spots &c. are compared is the value on an arbitrary scale of the deflection of the radio-micrometer when the centre of the solar image is allowed to fall into the pinhole aperture in the screen.

In taking an observation, the zero position of the spot of light on the scale when the pinhole is closed is first noted, and its value entered in column Z. The umbra of a spot is then placed, by means of the slow-motion handles, on the pinhole, which is then opened. The amount of deflection produced is then entered in column U. The image is then moved so that a portion near the spot, but at the same distance from the limb, falls into the pinhole. The deflection from this is entered in column N. And finally the solar image is placed centrally, and the radiation from the centre of the disc is entered in column C.

When the values in column Z are subtracted from those in U, N, and C, we get comparative values of the radiation from the umbra, the neighbourhood, and the centre of the Sun.

In a former paper † it has been shown that the deflections of the radio-micrometer are, at any rate up to 20°, strictly proportional to the amount of radiant energy falling on the instrument. We are therefore authorised in taking these values as

* *Proceedings of the Royal Society*, vol. lv.

† "Effective Temperature of the Sun," Wilson and Gray, *Phil. Trans.* vol. clxxxv. A.

equal to the amount of radiant energy reaching us from the different parts of the Sun. It has long been known that the radiation from the limb of the Sun is much less than from the centre, and that this is probably caused by a layer of what we may call smoke, which lies over the photosphere. It seemed to be an interesting point to investigate whether the radiation from a Sun spot falls off when near the limb in the same manner as a point in the photosphere, and since the commencement of these observations this point has been investigated. In the preliminary note published in the spring of 1894 I showed from observations then recorded that the radiation from the umbra of spots does *not* suffer absorption when near the limb, in the same manner as a point on the photosphere. These observations have been continued during the summer and autumn of last year and confirm the earlier ones.

In 1874 and 1875 Langley, * by means of a thermo-pile and galvanometer, measured the radiation from Sun spots. The mean value that he gives for the umbral radiation is 0·54. Instead of taking the radiation from a point in the centre of the disc as his standard, Langley took two points on the photosphere, the first situated between the spot and the centre, and the second between the spot and the limb. The mean of these two photospheric readings he then used as a divisor for the umbral reading. Langley seems to have been unable to observe that the radiation from the umbra of a spot is nearly constant for different distances from the centre of the disc. He says, "The decrement of heat as we approach the limb is, though not exactly, yet so very nearly, in the same ratio for photosphere and spots, that no correction is needed on this account." An inspection of the values of $\frac{U}{N}$ in Table I. clearly shows that the radiation from the umbra of a spot, as its distance from the centre increases, becomes closer to the value of the radiation from the neighbouring photosphere. Whereas the values of $\frac{U}{C}$ remain nearly constant. As Langley's observations were taken near a solar minimum year and mine at a solar maximum, it is just possible that the radiation from spots may vary during this period. Future observations taken throughout a complete sun-spot cycle may show that the spots are formed during the solar minimum at a lower level, and thus suffer more absorption near this limb than they do during a solar maximum.

I have collected the monthly means of my observations from August 1893 to Nov. 1894, and plotted out the values of $\frac{U}{C}$ in the following curve :—

* *Monthly Notices*, vol. xxxvii. No. 1.

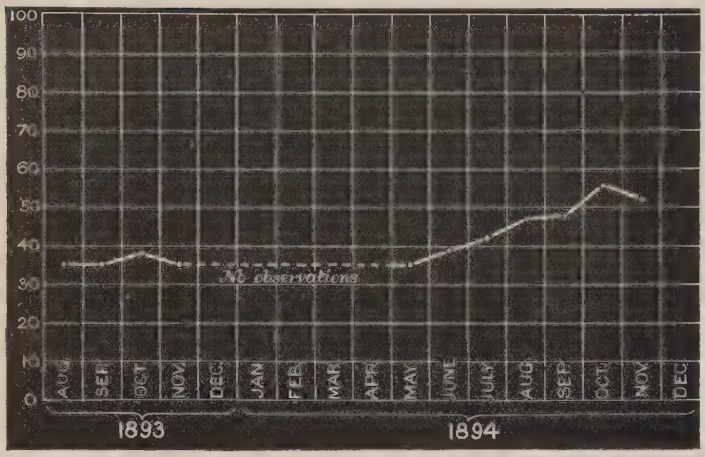


Fig. 1.—Radiation from an equal area of the photosphere at the centre of the disc = 100.

It will be seen that the umbral radiation is considerably lower during 1893, and since May 1894 the values of $\frac{U}{C}$ have increased from 0·35 to over 0·50.

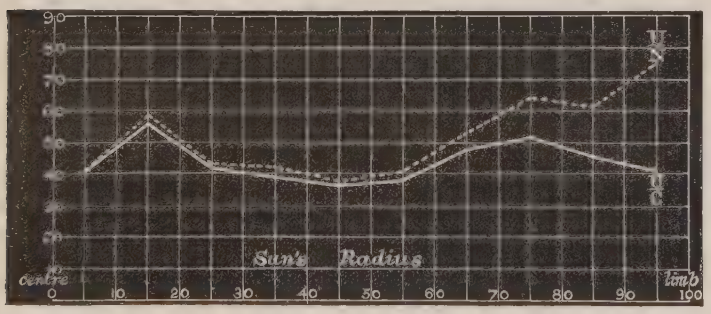


Fig. 2.—Radiation from an equal area of the photosphere at the centre of the disc = 100.

In fig. 2 I have collected the values of $\frac{U}{C}$ and $\frac{U}{N}$ according to distance along the Sun's radius. The continuous curve gives the values of $\frac{U}{C}$, while the dotted one gives those of $\frac{U}{N}$, and shows how the latter departs from the former as the limb is approached.

Since I published my first observations my attention has been drawn to a paper on this subject by Professor Frost, of

Dartmouth College, U.S.A. He there points out the same fact as I have—that the light from umbra of spots does not suffer absorption when they are near the Sun's limb in the same manner as a point in the photosphere. He also records some observations in which he found the radiation from the umbra greater than that from the neighbouring photosphere when the spot was close to the limb. I have never been able to observe this. The greatest value for $\frac{U}{N}$ that I have recorded is 0·83.

In Table I. are given the ratios of the radiation from the umbra (U), the neighbouring photosphere (N), and the centre (C). In column D is given the distance of the spot from the Sun's centre, in hundredths of the radius. Where observations are bracketed together it signifies that it is the same spot that has been observed on different days. Unfortunately, in this climate we are very seldom able to observe a spot for many days in succession.

TABLE I.

Date.	$\frac{U}{C}$.	$\frac{U}{N}$.	D.	Remarks.
1893.				
Aug. 5	0·37	0·43	60	
7	0·29	0·29	40	
8	0·29	0·32	50	
8	0·34	0·38	40	
8	0·42	0·51	90	
14	0·36	0·37	50	
19	0·37	0·37	50	
Sept. 2	0·31	0·31	10	
3	0·30	0·30	10	
4	0·42	0·45	30	
4	0·43	0·45	30	
7	0·29	0·35	85	
Oct. 1	0·40	0·40	30	
1	0·49	0·57	80	
22	0·34	0·35	52	
26	0·36	0·41	40	
29	0·32	0·71	90	
30	0·36	0·78	95	
30	0·58	0·65	...	No real nucleus.
Nov. 8	0·36	0·80	97	
9	0·34	0·85	92	

Date.	U_C.	U_N.	D.	Remarks.
1894.	′	′		
May 30	0·34	0·34	...	Spot A.
30	0·32	0·34	57	Spot B
31	0·33	0·34	45	Spot A.
31	0·39	0·40	42	Spot B.
June 6	0·38	0·54	86	
30	0·41	0·61	89	Spot A.
30	0·62	0·66	...	Spot B. No definite umbra.
July 1	0·43	0·76	97	Spot A. Spot B nearly filled up.
21	0·61	0·64	60	Sky hazy.
21	0·46	0·52	58	
30	0·42	0·47	65	
30	0·38	0·78	94	
30	0·39	0·67	92	
Aug. 6	0·49	0·51	41	
22	0·46	0·70	96	
Sept. 8	0·42	0·58	74	
9	0·47	0·51	61	
10	0·43	0·45	41	
13	0·47	0·47	...	Spot nearly central.
14	0·45	0·50	53	Same spot as observed on 8th, 9th, 10th, 13th.
29	0·58	0·66	83	
30	0·55	0·61	67	
Oct. 4	0·60	0·66	72	
4	0·56	0·59	19	
4	0·55	0·64	84	
7	0·54	0·64	...	Clouds.
19	0·53	0·83	96	
Nov. 5	0·51	0·52	57	
5	0·61	0·61	5	
5	0·49	0·71	84	
8	0·42	0·83	90	
8	0·54	0·69	72	
8	0·58	0·60	40	
30	0·48	0·80	94	
30	0·50	0·78	83	

A Negative Optical Proof of the Absence of Seas in Mars.

By H. Dennis Taylor.

When occupied in reading a highly interesting article on the physical configuration of *Mars* in *Astronomy and Astrophysics* an idea suddenly struck me which seemed to me a valuable one —viz. that if there exist seas, or even water-covered spaces of moderate size, within the tropical regions of *Mars*, then we should expect to see a minute but brilliant reflection of the Sun in such water spaces whenever, by the diurnal rotation of the planet, they were brought into the proper angle to send us a solar reflection. I at once entered into a few simple calculations which satisfied me that the apparent brightness of this solar reflection, analogous to a reflection of the Sun from a thermometer bulb, would at least compare on fairly equal terms with the brightness of a 1st magnitude star. I then drafted a paper on the subject and submitted it to the March Council meeting of this Society, along with a note in which I said I could scarcely believe that its matter was entirely novel, and that the idea had not occurred to anybody before. At the same time I was not aware of any books on astronomy in which the important bearing of this question upon the physical state of *Mars* was even alluded to. Since then, however, one of the referees to whom my paper was submitted has kindly referred me to the memoir of Schiaparelli on *Mars* and to a paper dealing with this very question by Professor Phillips, of York, which, it seems, was published first in the *Proceedings of the Royal Society*, vol. xii. 1863. So that the claim for priority rests, I believe, with Professor Phillips. But until the other day I was quite ignorant of this, and the fact that the very same idea should afterwards recur to me, living in the same town, seems to suggest that there is something in ideas akin to certain epidemics. In the course of discussing the nature of the markings upon *Mars*, Professor Phillips wrote : "A positive proof of ocean on the disk of *Mars* would be afforded by the star-like image of the Sun reflected from the quiet surface or the more diffused light thrown back from the waves ; but nothing of this sort has been placed on record." In a note he also wrote : "The quiet image here alluded to would not exceed $\frac{1}{20}$ of a second of angle at the opposition, if no allowance be made for irradiation." He then concludes by assuming it doubtful whether this star-like image would be visible except perhaps in very large instruments. But he enters into no calculation as to the apparent *brightness* of this "artificial star" as viewed from the Earth and as compared with other real stars.

Then Professor Schiaparelli, in his beautiful and elaborate memoir on *Mars* during the opposition of 1877, took up the subject again as being of the very highest importance. While expressing himself as very doubtful as to whether the darker

markings of *Mars* are to be regarded as seas, he then passes to the consideration of what he seems to consider the most powerful argument against their existence. I will here give a condensed translation of his method of dealing with the question. His figures are in practical accordance with what I have myself arrived at as most probable.

"It is certain," he says, "that if *Mars* were a polished sphere, capable of reflecting, in the fashion of a speculum, the solar rays, we ought to see a very minute image of the Sun glittering on his disc in the form of a star, and at a position easy to assign at each moment. Assuming the reflection to be total, it is easy to calculate the intensity of this image, given the diameter of *Mars* and its distance from the Earth and Sun. For example, in the opposition of 1877 attained on September 5, *Mars* being at the distance of 1·383 from the Sun and about 0·377 from the Earth, the image in question ought to figure as a star of a diameter of $\frac{1}{2\frac{1}{4}}$ of a second of arc, and therefore of a brilliancy equal to $\frac{1}{2,100,000,000}$ of that of the Sun at distance 1. Now Professor Zöllner has estimated the total light reflected to us from *Mars* in mean opposition to be $\frac{1}{6,994,000,000}$ of that of the Sun at unit distance. From this it follows that in the opposition of 1877 the total luminosity of *Mars* was $\frac{1}{2,990,000,000}$ of that of the Sun at distance 1. Therefore, assuming total reflection, the minute solar image reflected from the polished surface of *Mars* would surpass in brilliancy the whole of the light from his disc. However, great allowance must be made for the low reflective power of a liquid like water, whose index of refraction being only $\frac{4}{3}$ would imply a reflective power for approximately perpendicular incidence of about $\frac{1}{49}$ part. Then allowance must be made for the absorption due to the double passage of the solar rays through the Martian atmosphere. Assuming the absorption to be as great as in the case of our own atmosphere, the above fraction ought to be reduced to about one-half, or in round numbers $\frac{1}{100}$. On this supposition, then, we should have for the luminosity of the reflected image

$$\frac{1}{21 \times 10^{10}}$$

of that of the Sun. But Zöllner has estimated the brightness of *Capella* to be

$$\frac{1}{5{\cdot}57 \times 10^{10}}$$

of that of the Sun. So that on 1877 September 5 the image of the Sun reflected from the convex mirror formed by any seas in *Mars*

ought to have appeared of a brightness equal to about one-fourth of that of *Capella, and therefore as a star of the 3rd magnitude.*"

The last words in my own italics are remarkable as seeming to show that Professor Schiaparelli regarded *Capella* as of the 1st magnitude, whereas measurements have made it of zero magnitude, or two and a half times as bright as an average 1st magnitude star. Therefore Professor Schiaparelli's magnitude 3 ought to read as magnitude 1·5. He then goes on to say that "Such a star could not possibly escape observation if occurring upon any of the darker areas of *Mars*, in spite of the undoubted influence which the surrounding bright surface of the planet would have to drown its splendour. But very slight inequalities in the surface of the water, in the shape of waves, would be enough to break up the clear united reflection into an infinitude of minute reflections. Now it is true that the sum of the luminous values of these reflections would not turn out much different to the single image, but the light would be spread over a larger space, and the amount of extension would depend upon the form of the waves and the inclination of their sides to the horizon. The formation of rugged crests would destroy any real image, and the nebulous patch of light having but a fraction of the luminous value of the light from the disc, might easily escape observation." Seemingly, then, Professor Schiaparelli concludes that the solar reflection could only fail to be visible to a spectator on the Earth armed with a good telescope, by reason of the water areas of *Mars* being in a rough and turbulent state.

But it would have to be assumed that the Martian water surfaces are *always* rough, and have always been rough ever since the invention of the telescope, which supposition is, I venture to think, quite untenable, if not ridiculous, in face of all we know about our own oceans, lakes, and rivers, and also certain facts, more clearly established of late, with regard to the behaviour of the so-called seas of *Mars* and the density of the Martian atmosphere.

Before returning to the discussion of the question, of the utmost degree of roughness allowable in the water areas of *Mars* consistently with the visibility of the solar reflection, I propose to slightly supplement Professor Schiaparelli's calculations of the brightness of the solar reflection by a different method.

Such various determinations have been arrived at by different observers as to the stellar magnitude of the Sun, or the ratio between its apparent luminosity and that of a star of zero magnitude like *Vega* or *Capella*, as to raise some doubts as to the degree of accuracy to be assigned to the above calculations, although the photometric estimates of the Sun's brightness arrived at by Zöllner certainly seem to accord fairly well with the apparent brilliancy of the planets.

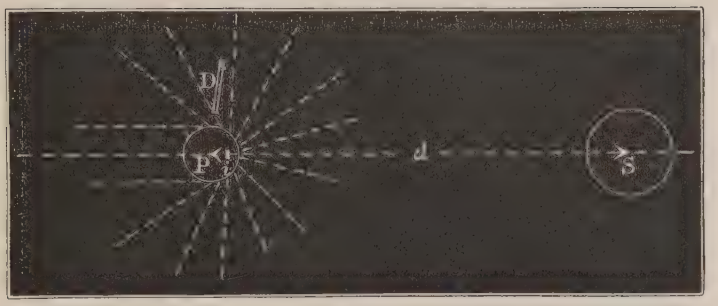


Fig. 1.

In fig. 1 let S be the Sun of a radius R, P a polished sphere of radius r illuminated by S at the distance d separating centres of S and P. And let D represent a disc, also of a radius r equal to that of P, which is placed at the same distance d from S, and illuminated perpendicularly by the latter. Let the surface of D be supposed to be of a pure white light-scattering character, reflecting irregularly all the light which falls upon it, and also let P be supposed to reflect, as a convex speculum, all the light which falls upon it. In other words, let their albedoes = 1 in both cases. Then it is obvious that D and P receive equal amounts of light from S, and since they reflect it all off again, in one case by irregular reflection, and in the other case by specular reflection, I was misled at first into supposing that, to an observer situated at any point on the line P - - - S, the point of light seen by reflection on P must be equal in total intensity to the total intensity of the light received from D.

But a little consideration is quite sufficient to throw doubt upon such a conclusion, for the disc D throws all the light back through an angle of 180° to the front of itself, and cannot throw any light behind its own plane, but the light reflected from the polished sphere P is reflected off through an extent of angle of 360°, and this distribution of light through greater angle must necessarily be at the expense of intensity of reflection through the forward 180°, and most likely also at the expense of intensity of the reflection in the direction of P - - - S. However, this can be easily put to the test of more exact calculation.

Let

B = intrinsic brightness of globe S

R = radius of globe S

d = distance representing centres of D or P and S.

Then the apparent intrinsic brightness of D, having an albedo = 1, will be

$$B\frac{R^2}{d^2}.$$

If now L represents the total apparent brightness of S as viewed from a standard distance d, then the apparent total brightness of D as viewed from S (at the same standard distance d) will be equal to

$$L\left(\frac{R}{d}\right)^2\left(\frac{r}{R}\right)^2 = L\left(\frac{r}{d}\right)^2.$$

Turning now to the case of the polished sphere P we have a miniature image of S formed at i halfway between the centre and periphery of P, and this image subtends the same angle at the centre of P as does the real source of illumination S, and also, by a well-known law of optics, the intrinsic brightness of this miniature image is the same as that of the original S. Therefore the apparent total brightness of the image i as viewed from the standard distance d will be equal to

$$L\left(\frac{\frac{1}{2}r}{d}\right)^2 = L\left(\frac{r}{d}\right)^2 \cdot \frac{1}{4}$$

or one-fourth of the value found for the light from the disc.

I have verified this theorem by a few simple experiments, as follows. A thermometer bulb full of mercury, the diameter of the mercury globe being ·485 inch, was compared with a disc of white paper of the same diameter, both being illuminated from the same source and viewed from a point 20 to 30 feet away. In fig. 2 C is a candle flame, S an opaque screen, B the mer-

Fig. 2.

cury bulb, D the disc of white paper, and O the position of observation, not less than 20 feet from C or D. The disc was placed to perpendicularly face the light, and both disc and bulb placed as closely as possible to the line of observation C - - - O. The observations were of course made in a darkened room. At the distance C - - - O the disc and bulb presented no very sensible diameter. Then, being short sighted, the light both from the disc and from the specular reflection of the candle flame in the bulb were spread out into a penumbra on my retina, and by a further voluntary shortening of the focus of my eye I could swell out the two lights to be compared, D and B, into two slightly overlapping discs whose apparent diameters so far exceeded the apparent real diameter of D as seen from O as to render the correction due to the greater area of D as small as possible. As the penumbras were generally made to be about ten times the apparent diameter of D, then the areas of the two

apparent lights became 10^2 for D and 9^2 for B, or about 5 to 4. B was placed at from $1\frac{1}{2}$ inches to 3 inches from the candle in different experiments, and then the disc D was adjusted to that distance from the candle found necessary to make the penumbras from the two lights appear of equal intensity as viewed from O.

In three experiments equality was attained when the ratios of the squares of the distances of B from C and D from C respectively were 1 : 3·6, 1 : $4\frac{1}{3}$, and 1 : 3·36.

In another case, with a different mercury bulb in use, the ratio came out about 1 : 4. With 1 to 3, D appeared slightly but distinctly the brightest; with 1 to 5·7 B was distinctly the brightest.

In two other cases a bulb of freshly polished speculum metal of ·37 diameter was tried against a disc of pure white paper of the same diameter, and the results again averaged 1 to 4 or rather under. Zöllner estimated the albedo of white paper to be ·70 (*Photometrische Untersuchungen*, p. 273) and mercury ·65, while Lord Rayleigh has estimated the albedo of mercury to be ·75 at nearly perpendicular incidence (*Phil. Mag.*, October 1892); so I think it will be fair to assume the albedoes of the bulb and of the white paper about equal, or perhaps the reflective power of the mercury slightly lower owing to it being encased in a thin glass bulb. A careful comparison of the two mercury bulbs used and the speculum bulb, as regarded their power of reflecting a sheet of white paper, showed no sensible difference, and speculum metal has been estimated by Sir John Conroy and others to prove an albedo varying between ·60 to ·70. These simple experiments, while not pretending to great accuracy, certainly confirm the theorem that the light reflected back from a polished sphere along the direction of illumination is one-fourth of the light reflected back in the direction of illumination from a disc having the same diameter and perpendicularly illuminated by the same source at the same distance, and supposing the two albedoes are equal. We next have to consider the modification of the amount reflected back when a spherical light-scattering surface is substituted for the flat light-scattering surface of the disc, the diameter of course remaining the same.

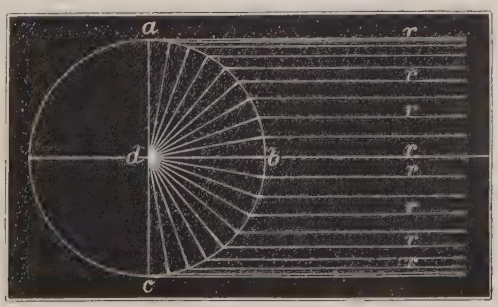


Fig. 3.

Let r, r, r, &c., in fig. 3 be parallel rays from the distant source of illumination falling upon the sphere a, b, c. Instead of a disc a - c receiving all these rays at perpendicular incidence, we must treat the surface of the hemisphere as cut up into a number of equal zones having their edges parallel to the plane or disc a - - c and having the common axis d - - b, which zones get illuminated at different degrees of incidence, and whose apparent widths and areas as viewed from the direction r - - r show large variations.

If the quadrant a - d - - b is divided into n equal parts, and the area of the base (or disc a - c) or $r^2\pi$ is supposed to equal 1 and to reflect light equal to 1 along the direction d - - r, then we have the sum of the following series as representing the total apparent luminosity of the whole hemisphere as viewed from a distant point on d - - r :

$$r^2\pi\Big[\Big\{1-\sin^2\Big(\frac{n-1}{n}\Big)90^\circ\Big\}\cos\Big(\frac{n-\frac{1}{2}}{n}\Big)90^\circ + \Big\{\sin^2\Big(\frac{n-1}{n}\Big)90^\circ-\sin^2\Big(\frac{n-2}{n}\Big)90^\circ\Big\}\cos\Big(\frac{n-1\frac{1}{2}}{n}\Big)90^\circ+\&c.,$$

&c., to n terms. And, if $r^2\pi = 1$, the total value of the series is $\frac{2}{3}$. This agrees perfectly with Zöllner's two theorems ; first as to the apparent illumination of a sphere being equal to that of a cylinder of same radius but of a height equal to $\frac{2}{3}$ of the diameter of the sphere, and having its axis perpendicular to the direction of viewing and illumination ; and, second, the apparent illumination of a cylinder with its height *equal* to its diameter, and axis perpendicular to direction of viewing and illumination being equal to the apparent illumination of its circular base when perpendicularly illuminated. Therefore the apparent brightness of the solar reflection off water surfaces in *Mars* in terms of the total apparent brightness of *Mars* will be

$$\frac{3}{2}\cdot\frac{1}{4}\cdot\frac{a}{A}F$$

where

a = the albedo of water

A = the general albedo of *Mars*

and

F = that fraction of the total incident light which is left after two passages through the Martian atmosphere.

Here A and F are both uncertain factors. Zöllner gives ·267 as the general albedo of *Mars*, but Seidel gives only ·09 or so ; while Professor Pickering, as the result of careful photometric measurements of the relative brightness of *Mars* and *Saturn* when at or

near conjunction in 1877, came to the conclusion that the albedo of *Mars* was not more than $\frac{1}{4{\cdot}6}$ of that of *Saturn*. If, therefore, we suppose the albedo of *Saturn* to be as high as that of freshly fallen snow, which Zöllner estimated as ·78, then the albedo of *Mars* comes out ·17. I will in the above formula assume ·21 to be the correct albedo for *Mars*.

The albedo of water for all angles of incidence up to about 20° has been proved to be about ·021 (Lord Rayleigh, *Phil. Mag.*, October 1892), and I will assume the fraction of light left after two passages through the perpendicular thickness of the Martian atmosphere to be $\frac{2}{3}$, in view of the strong evidence existing to show that its atmosphere is considerably thinner than our own. Then the above formula gives a value of $\frac{1}{40}$ as the ratio between the intensity of the solar reflection from a water surface in *Mars* and the total brightness of the whole Martian disc. Now Zöllner's photometric measurements of the apparent brightness of *Mars* gave it a relative brightness as compared with *Capella* of 8 to 1 when *Mars* is in mean opposition. But *Capella* is of 0 magnitude, therefore *Mars* in mean opposition is of an apparent brightness equal to twenty times that of a 1st magnitude star. Therefore the solar reflection from water in *Mars* at mean opposition should have an apparent brightness equal to half that of a 1st magnitude star, or $\frac{1}{8}$ of *Capella*. Schiaparelli's calculations, as we have seen, made it out to be equal to $\frac{1}{4}$ of *Capella*, with *Mars* in close opposition.

But a star equal in brightness to half a 1st magnitude star is a brilliant and conspicuous object even in a 2-inch telescope. Here a simple experiment can be made very useful and suggestive. I took an empty thermometer bulb made of thin opal glass measuring 1·4 inches in diameter. This glass is of the ordinary refractive index of about 1·53 or 1·54, and is impregnated, especially towards the inner surface of the bulb, with some white light-scattering substance which gives the whole bulb a resemblance to a globe of milk. On comparing its albedo or light-scattering power with that of pure white paper it seemed rather inferior, so that I should judge the albedo of the bulb to be about ·5. But the polished *surface* of the bulb gave a specular reflection of the source of illumination, and the amount of light reflected in this way would be that usually reflected by a glass surface. Its specular reflection was compared with, and found equal in intensity to, that from the first surface of a crown glass lens whose other surface was backed up by white paper. For an index of 1·54 the amount reflected at approximately perpendicular incidences would be represented by Young's formula

$$\left(\frac{\mu-1}{\mu+1}\right)^2=\left(\frac{\cdot54}{2\cdot54}\right)^2=\cdot045.$$

And if any of the white light-scattering particles happened to lie in or upon the actual glass surface they would obviously diminish

the amount of light reflected regularly, so that $4\frac{1}{2}$ per cent. represents the utmost amount of the incident light which is regularly reflected. This white bulb was then placed in a dark room and illuminated by a candle flame placed six inches to a foot or more away and as nearly as possible in the line of sight, the flame being screened in the direction of observation. On receding to some distance a white globe was visible in which the minute image of the candle flame stood out boldly like a star, and this star was too conspicuous to escape naked-eye vision at a distance of 30 feet, where the bulb would subtend about 13 minutes of arc. Here the relation between the albedo of the white surface and the true glass surface giving the specular reflection was $\frac{\cdot 50}{\cdot 045}$, or about 11 to 1, whereas in *Mars* we concluded that the relation between the general albedo of its surface and the albedo of water was $\frac{\cdot 21}{\cdot 021}$, or 10 to 1; or 15 to 1 after allowing for an absorption of $\frac{1}{3}$ for the double passage of the solar rays through the Martian atmosphere. Therefore the two cases are not very materially different, and a magnifying power of about sixty on any good telescope, enlarging *Mars* to an apparent diameter of say 18 minutes, would be quite sufficient to render the solar reflection easily visible. Yet, however difficult it may be to suppose that the water surfaces of *Mars* are never calm enough to give a defined reflection of the Sun, it is nevertheless advisable to consider the effect of moderate waves upon the image. To *Mars* at mean distance the Sun subtends about 21 minutes of arc, and its image reflected off a spherical water surface in *Mars* would appear to lie halfway between the centre and circumference of the planet, and would subtend 21 minutes of arc at *Mars'* centre. Therefore the angular diameter of the solar image in *Mars* as viewed from the Earth at average opposition would be, if the water surface were still, represented by

$$\frac{d}{4}\theta$$

where d = the apparent diameter of *Mars* in seconds and θ = the circular measure of 21 minutes. This comes out only ·0275 seconds of arc, or so small that it would need a telescope of 15 feet aperture at least to show any appreciable disc.

The absolute size of the solar image in miles would be equal to one quarter of the Martian diameter multiplied by θ or $1050.\theta$; that is, about $6\frac{1}{2}$ miles. Therefore lakes as small as this, if they occurred within the Martian tropical zone, should be capable of throwing us an unimpeded reflection of the Sun. But $6\frac{1}{2}$ miles is far narrower than the finest of the so-called canals of *Mars*. But we have never seen any flash of sunlight from them. It must be remembered too that small sheets of water are not so likely to be troubled with waves as oceans, and even if perturbed, subside again into calmness much sooner.

Let it be supposed, however, that the water surfaces were cut up into waves, whose sides at their greatest slope were inclined at an angle β to the Martian horizon. Then the effect would obviously be to spread out the solar reflection over an angular extent of the Martian surface equal to 2β. How much would it have to be spread out before it would quite escape scrutiny? This cannot be answered except in very general terms. Experiment leads me to think that an inclination of the steepest part of the wave to the horizon of about $14\frac{1}{2}°$ may be possible before the diffused patch of reflected light scattered over 29° of Martian surface would cease to be visible in a powerful telescope giving a large and bright image of *Mars*.

I took the same opal bulb as before, and, instead of illuminating it by a small candle flame, I illuminated it by a square sheet of white paper, reflecting the candle light on to the bulb.

In this way the ratio between the total light scattered by the opalescent substance of the bulb and the total light regularly reflected by the glass surface in the form of a broad patch of light (being a distorted image of the white paper) would not materially be disturbed, but instead of a luminous point, a broader and less intense image was formed. And I found that even when the sheet of paper was large enough to give a reflection in the bulb equal in apparent diameter to one-fourth of the apparent diameter of bulb, the approximately rectangular patch of light showed up in a quite noticeable manner against the duller background. And I do not see any reason for supposing that the effect would be essentially different, if, instead of diffusing the illuminant itself, we diffuse the image of that illuminant by the presence of corrugations on the reflecting surface, having a maximum inclination of sides to the horizon of $14\frac{1}{2}°$, whose sine is $\frac{1}{4}$.

But such waves as these are only what would occur in a fairly rough, if not a very rough sea. Shallow seas do not develop great waves so readily as deep ones, and what waves do arise soon subside again. Some recent telescopic observations of *Mars*, made under the most favourable atmospheric conditions prevailing at the loftily situated observatories in America, notably Mr. Percival Lowell's observatory in Arizona, certainly confirm very strongly the great variability in the outlines and depth of colour of the so-called seas of *Mars*. One cannot read Mr. Percival Lowell's articles on *Mars* in *Astronomy and Astrophysics*, detailing the great changes which he observed to take place as the summer advanced in the southern hemisphere of *Mars* during the late opposition, without being struck by the fact that the darker greenish markings of the disc, from being scarcely discernible and of generally small extension at the spring equinox, develop steadily both in depth of tint and extension to a most marked degree until what we have every reason to regard as the hottest part of the Martian summer is past, when they shrink in size and fade in colour until in the autumn

they have again relapsed into comparative obscurity. And this takes place in an even more marked degree in the case of the "canals."

If these dusky markings are really seas, then it is very obvious that they must be for the most part extremely shallow, or their extraordinary variations in area cannot be accounted for. And if shallow, then we have little reason for supposing them to be generally much agitated. Besides, even supposing that the atmosphere of *Mars* contains as much gas per square mile of surface as our Earth does, yet its density or pressure would be only two-fifths of that of our atmosphere, while its vertical movements under variations of temperature would have to take place on many times the scale of our Earth in order to produce the same amount of cooling and storm-generating effect. And, although the weight of water would be only two-fifths, yet its inertia and internal friction would be the same as with us, while, for a given wind velocity, the wind *pressure* would be considerably less than with us. Altogether it is extremely probable that the atmospheric disturbances of *Mars* are very mild affairs compared with our own. Mr. E. W. Maunder has very ably discussed the question of the climate of *Mars* in *Knowledge*, vol. xv., and arrived at much the same conclusion. We have therefore good reason for expecting any water surfaces existing on *Mars* to be generally in a calmer state than on our own planet.

Yet it is a very common thing in the case of our own sheets of water to see the Sun's image reflected with no greater unsteadiness than will cause oscillations of its image through five to ten times its own apparent diameter, and this amount of movement would, in the case of the Martian solar reflection, make no perceptible difference to the image as seen in an 18-inch refractor ; it would still appear as a star.

Now some of the most well-marked "seas" of *Mars* happen to be well situated for sending us reflections when the Martian South pole is tilted towards us during a favourable opposition, like last year's ! From Schiaparelli's *Mare Cimmerium* (longitude 200) eastwards to the E. border of the *Auroræ Sinus* (longitude 60) for 220 degrees of longitude altogether, there is an only slightly interrupted series of so-called seas within a belt lying between 20° and 30° of South latitude, and therefore coming about Martian midday into the plane containing the centres of the Sun, *Mars*, and the Earth. But not a trace of a glint seems to have ever been observed.

The Martian atmosphere cannot be supposed to appreciably interfere with the visibility of the solar reflection, if it exists. I have allowed for an atmospheric absorption of one-third of the light in the above calculations. For, while the planet's atmosphere may absorb more or less light in the sense that glass does, yet we cannot suppose that it also scatters and diffuses light, and thus interferes with the definition of details seen

through it, as our own atmosphere does on a hazy day. For, during the summer, at any rate, of *Mars'* southern hemisphere, the visibility of such extremely fine details as the more difficult "canals" of *Mars* is fairly constant, and depends altogether upon our own atmospheric conditions being favourable.

The dark zone which so closely follows up the retreating edge of the S. polar cap of *Mars* as the summer advances, has usually been considered to be a belt of water resulting from the thawing of the polar snows. But what proof have we that the white polar cap consists of snow of sufficient depth to cause such extensive inundations as would be indicated by the dark-greenish areas lying around the S. pole, supposing they are water surfaces? The rapid melting and shrinkage, and in some cases total disappearance, of the polar snows each summer, is in itself strong evidence of the small depth of the snow deposit (or hoarfrost deposit, or whatever it is), for the efficiency of the Sun's heat for melting snow in *Mars* is barely a half of what it is with us. If we suppose,* then, that the dark-greenish areas which develop and spread in the S. polar regions of *Mars* as the summer progresses are really sheets of water resulting from the thaw, we must obviously assume a most extraordinary degree of flatness in the surfaces inundated, for we could not assume more than two or three feet of depth of water on the above hypothesis, as the greenish areas around the pole far exceed the area of the snow cap, which in itself cannot be more than a few yards thick.

The hypothesis which, I venture to think, best accords with the facts is suggested by the case of our own Earth, where a zone of revivifying vegetation of a dwarf character closely follows up the melting of the snows which have formed a temporary extension of the true polar ice cap during the winter. It is this zone of vegetation, within and about the Arctic circle, which annually becomes the principal breeding ground of a very large number of migratory birds, who find their food either in the herbage and vegetation itself or else in the insects, &c., which live among it. Such a zone existing in *Mars* could scarcely escape observation as a region of a darker and more greenish colour than the rest of the surface. And, seeing that we have negative proof of the fact that the areas having the same colour and appearance, but lying within the planet's tropical regions, are not water surfaces, we may assume as most likely that the greenish colour of all the darker markings of the planet is caused by the presence of vegetation growing in the more low-lying regions where there is sufficient moisture existing in the soil, while the brighter and ruddy-tinted regions are barren desert. The observed seasonal changes in the markings would thus be accounted for.

The Rev. Edmund Ledger, in a recent lecture, discussed the

* Obviously, we could not apply the test of presence or absence of a solar reflection to sheets of water outside the tropical regions of the planet.

absurdity of supposing that the canals could be looked upon as artificial waterways in themselves, for they must be at least 20 miles in diameter to be discerned at all, but he suggested that they might indicate the lines of canals, the visible broadening out being due to cultivated tracts on either side of the main waterway, which should show a darker and greenish tinge. But, if not canals, they might indicate the lines of roads cut straight across the deserts from one centre of population to another. It would be obviously the aim of any inhabitants to make the roads as straight as possible in a nearly waterless planet. Then, if tracts of country were cultivated and made to support vegetation, surely the districts immediately surrounding the centres of population (the dots seen where the canals intersect) would first be attended to, and afterwards substantial strips of desert at each side of the roads would be reclaimed and covered with vegetation. This is one hypothesis which might help to explain the suspiciously artificial-looking and designed appearances of the so-called canals of *Mars*.

The remaining question, of the relative brightness of the solar reflection when *Mars* is in other positions than in opposition, may be shortly dealt with ; for the relative configuration of the orbits of the Earth and *Mars* is such that two lines, one joining the Sun and *Mars* and the other joining the Earth and *Mars*, never enclose a greater angle than about 50°, so that the maximum angle of incidence and reflection would be 25°. But it has been proved that the percentage of light reflected from water at that angle is not very appreciably greater than that reflected perpendicularly. The contrast between the solar reflection and the gibbous disc of *Mars* would thus remain much about the same as when in opposition.

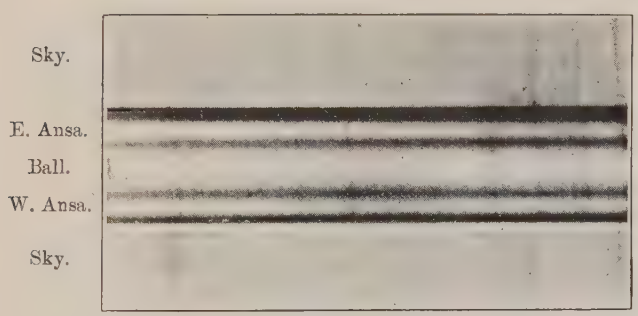
Photographs of the Spectrum of the Ball and Rings of Saturn.

(*Letter to the Secretary.*)

E. W. Maunder, Esq.,
Secretary of the Royal Astronomical Society,
London, England.

Dear Sir,—I send you with this a photograph of the spectrum of *Saturn*, which it may interest some of the Fellows of the Society to examine. The fact that the lines in the spectra of the ansæ do not follow the direction of the planetary lines, but are very slightly inclined in the opposite direction, is a spectroscopic proof of the meteoric constitution of the ring, as I have explained at length in the May number of the *Astro-physical Journal*. Although the positive enlargement which I send is less satisfactory than the original negative viewed under a microscope, the feature mentioned above can be seen without difficulty.

To prevent the eye from being influenced by the slope of the planetary lines, it is best to cover the central spectrum with a narrow slip of paper.



Spectrum of Saturn, 1895 April 19.
Enlarged 15 diameters from the original negative taken at the Allegheny Observatory, by J. E. Keeler.

The large relative displacement of the planetary and sky spectra is due to the fact that the comparison spectrum was photographed the next morning after the exposure to the planet, and at a quite different temperature. It does not affect the value of the photograph for this particular purpose. The exposures were, *Saturn* 2 hours, and sky 4 minutes. As the sky spectrum was too dense, a bar was placed across the plate during part of the exposure (in making the positive), and some horizontal markings resulted, which are not on the original negative.

Yours very truly,

James E. Keeler.

Observations of Jupiter. By W. F. Denning.

The following are a few results of observations of *Jupiter* made between 1894 November 5 and 1895 May 10 with a 10-inch reflector, powers 252 and 312.

The red spot was seen on several occasions, but it was extremely faint, especially at its preceding end. Its south side appeared to be connected with a grey belt. Between 1894 November 25 and 1895 May 10 I found a mean period of

$9^h\ 55^m\ 41^s{\cdot}2$ (401 rotations).

This is slightly more ($0^s{\cdot}6$) than the period ($9^h\ 55^m\ 40^s{\cdot}63$) on which Mr. Marth's zero meridian System II. is based, and it

agrees exactly with the rate I derived from the preceding opposition. According to my last observation on 1895 May 10 the spot followed the zero meridian 8 minutes, so its longitude was 4°·9.

In north latitude about 35° there has been visible a narrow, dark, broken belt, and black spots have sometimes been seen upon it. I watched two of these objects between 1894 December 4 and 1895 January 28, and determined their periods as follow :—

	h	m	s	
(1)	9	55	39·5	(118 rotations)
(2)	9	55	38·5	(„)

The mean of the pair is $9^h\ 55^m\ 39^s$, or $2^s{\cdot}2$ less than that of the red spot. The markings on this northerly belt would therefore occupy about 18 years in completing one revolution of *Jupiter* relatively to the position of the red spot.

The great north equatorial belt is double, and its north side has exhibited a very disturbed condition in recent years. A numerous array of white and dark spots are dispersed along the belt in an irregular manner. I obtained the times of transit of many of these objects, and the resulting periods of five white and four dark spots are as follow :—

White Spots.				*Black Spots.*			
h	m	s		h	m	s	
9	55	37·7	(406 rotations)	9	55	37·8	(447 rotations)
9	55	37·1	(249 „)	9	55	36·8	(389 „)
9	55	36·6	(389 „)	9	55	32·2	(63 „)
9	55	34·4	(442 „)	9	55	31·0	(254 „)
9	55	31·3	(104 „)				

The motions of the spots differ in individual cases, and the rate of the same objects is not consistently maintained during a long interval. The fluctuations of speed are too great to be satisfactorily accounted for by errors of observation. It will be seen from the list that the rotation period as derived from the different objects varies to the extent of $6^s{\cdot}8$, the time ranging from $9^h\ 55^m\ 31^s$ to $9^h\ 55^m\ 37^s{\cdot}8$. The mean of all is $9^h\ 55^m\ 35^s$, or $6^s{\cdot}2$ less than the period of the red spot.

With reference to equatorial spots not many conspicuous examples were observed in 1894 November and December, but in 1895 January they became numerous. Ill-health and interruptions from cloudy weather, however, prevented my following them up with sufficient closeness to get good rotation periods, as the objects varied rapidly in appearance, and it was not easy to identify them. In March I observed a white and black equatorial spot for several nights as follow :—

	White Spot on C.M.	Dark Spot on C.M.	Interval.
	h m	h m	m
March 22	8 47	9 2	15
24	10 4	10 15	11
31	9 27	9 35	8

The dark spot was rapidly gaining on the white, and on April 5 and 7 I looked for the objects again, but the aspect of the region had quite changed, apparently by the blending of the two spots. I obtained observations on succeeding nights, thinking it possible the dark spot might reappear to the west of the white one, but I failed to recover either of the objects with certainty.

Saturn.—During the first half of May I made observations of this planet on ten nights with a view to detect spots on the equator or belts. I employed one of Steinheil's "monocentric micrometer oculars" of $\frac{1}{4}$ inch equivalent focus (= power of 312), but detected no irregular markings on the disc. There was a narrow dark belt near the equator and a broad diffused band of shading in the north hemisphere, but no other details could be made out with the means employed.

Bristol: 1895 *May* 20.

Diameters of Jupiter and his Satellites observed at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

Diameters of Jupiter.

The following measures were made with the 28-inch refractor during the past winter. They were commenced on 1894 November 23, but the bad weather and frost prevented any work from 1895 February 5 to March 12, so that the series is not as complete as was wished. The diameters are reduced to mean distance 5·2028 (Leverrier), and the equatorial diameters are corrected for phase from Mr. Marth's Ephemeris, published in the Society's *Monthly Notices*. The following table exhibits the results found by each observer, in separate columns, and also distinguishes between the diameters obtained with different eye-pieces, those with the powers 450 and 470 being grouped together. It will be seen that there are indications of personality and also of a difference due to the power of the eye-piece employed.

Diameters of Jupiter.

Date. 1894.	Power of Eye-piece.	Mr. Dyson. Equat. Diam.	Mr. Dyson. Polar Diam.	Mr. Maunder. Equat. Diam.	Mr. Maunder. Polar Diam.	Mr. Lewis. Equat. Diam.	Mr. Lewis. Polar Diam.
Nov. 23	470	...	...	...	...	38·853	36·306
Dec. 18	470	39·052	36·308	...	...	38·889	36·120
1895.							
Jan. 1	670	38·494	36·175	...	...	38·610	36·124
9	470	...	...	...	...	37·860	35·827
18	670	...	...	37·977	...	...	...
20	450	39·169	36·511	...	...	...	...
21	450	39·435	36·625	...	...	...	...
22	670	...	...	...	...	37·791	35·753
26	670	...	...	...	...	37·931	35·783
Feb. 5	450	...	...	38·605	36·136	...	...
Mar. 12	670	...	...	...	...	37·618	36·143
13	670	...	...	...	...	37·483	35·880
17	670	38·521	36·184	...	...	...	...
18	670	...	...	...	...	38·411	35·553
22	670	...	...	...	...	38·103	35·922
26	670	...	...	...	...	38·356	...
31	670	38·981	36·352	...	...	...	...
Apr. 1	670	...	...	...	...	38·179	36·146
3	670	...	...	...	...	38·228	36·029
Mean	450	39·219	36·481	38·605	36·136	38 534	36·084
Mean	670	38·665	36·237	37·977	...	38·071	35·926
(450)–(670)		+0·544	+0·244	+0·628	...	+0·463	+0·158
Mean Diameter		38·942	36·359	38·291	36 136	38·303	36·005
Ellipticity			$\frac{1}{15·1}$		$\frac{1}{17·8}$		$\frac{1}{16·7}$

Using the observations of all three observers, the resulting equatorial diameter is $38''·407$, the polar diameter $36''·099$, and an ellipticity of $\frac{1}{16·64}$.

Professor E. E. Barnard, with the 36-inch Lick refractor, obtained for the equatorial diameter $38''·522$, and polar diameter $36''·112$.

Diameters of the Satellites.

The equatorial and polar diameters of the satellites were made in January and March of this year, the plan of the work being to obtain a long series of both diameters taken on the same evening with the view of detecting any possible ellipticity. To avoid any prejudice, the equatorial diameters of all four were measured before making the measures for the polar diameters. A very complete series has been obtained, notwithstanding the break in February, above mentioned, and the observations on the whole indicate a slight prolate ellipticity in J I. and J II., the polar diameters being the larger. In the table the column (E – P) gives the difference of the two diameters on each evening, the sums of the positive and negative residuals, and the mean difference.

Date.	Observer.	Power.	J I. Equat. Diam.	J I. Polar Diam.	J I. E−P.	J II. Equat. Diam.	J II. Polar Diam.	J II. E−P.	J III. Equat. Diam.	J III. Polar Diam.	J III. E−P.	J IV. Equat. Diam.	J IV. Polar Diam.	J IV. E−P.
			″	″	″	″	″	″	″	″	″	″	″	″
Jan. 1	L.	670	1·131	1·126	+ 0·005	0·856	0·964	− 0·108	1·814	1·315	+ 0·499	1·582	1·129	+ 0·453
9	L.	470	1·090	1·122	− ·032	0·710	0·74	− ·033	1·478	1·362	+ ·116	1·314	1·429	− ·115
21	D.	450	...	...	...	...	...	...	1·286	...	...	...	...	...
22	L.	670	1·027	1·053	− ·026		1 064	− ·126	1·300	1 416	− ·116	1·340	1·438	− ·098
26	L.	670	0·835	0·989	− ·154	0·872	0·919	− ·047	1·271	1·276	− ·005	1·308	1·343	− ·035
Mar. 12	L.	670	0·796	0·775	+ ·021	0·742	0·726	+ ·016	1·247	1·325	− ·078	0·996	1·018	− ·022
13	L.	670	0·915	1·139	− ·224	0 874	1·050	− ·176	1·271	1·608	− ·337	1·342	1·187	+ ·155
17	D.	670	1·333	1·164	+ ·169	...	...	...	...	...	...	...	...	...
18	L.	670	1·083	1·356	− ·273	0·946	1·274	− ·328	1 793	1·857	− ·064	1·446	1·558	− ·112
22	L.	670	0·950	0·991	− ·041	0·932	0·971	− ·039	1·482	1·624	− ·142	1·434	1·498	− ·064
26	L.	670	1·051	1·236	− ·185	1·181	1·085	+ ·096	1·548	1·339	+ ·209	1·403	1·511	− ·108
31	D.	670	1·359	1·318	+ ·041	1·160	1·176	− ·016	...	...	...	1·378	1·360	+ ·018
Mean Diameters			1 052	1·115	+ ·236 − ·935	0·921	0·997	+ ·112 − ·873	1·449	1·458	+ ·824 − ·742	1·354	1·347	+ ·626 − ·554
Mean Diameter			″ 1·084		″ − ·063	″ 0·959		″ − ·076	″ 1·454		″ + ·009	″ 1·350		″ + ·007

For comparison the following are appended :—

			J I.	J II.	J III.	J IV.
			″	″	″	″
1880	Chicago,	18½ inch	1·114	0·980	1·778	1·457
1893	Lick	36 „	1·048	0·870	1·521	1·430
1895	Greenwich	28 „	1·084	0·959	1·454	1·350

Royal Observatory, Greenwich :
1895 *June* 11.

On a Fixed System of Star Coordinates.

By R. H. M. Bosanquet, F.R.S.

Polaris is taken as pole of a system of coordinates. The distance of any star from *Polaris* is one coordinate (D). Great circles are drawn through *Polaris* and other stars, one of which (ε *Orionis*) is taken as starting-point. The angle at *Polaris* between any of these great circles and the one through ε *Orionis* is the other coordinate, and is called the ascension (A).

If consistent positions of the stars concerned are given in any system of coordinates, the *Polaris* distance and ascension can be found. Where the given places are equatorial, the work has been reduced to the determination of small quantities for stars of moderate declination ; and the calculation can be carried to the third decimal of 1″ with ordinary logarithms by a short process.

The following places of *Spica* are from a list reduced from the various catalogues. The annual change is by comparison with the Bradley place ; the number for 1900 by the annual change.

α *Virginis.*

Mean Places.	A. 117° 12′	Annual Change.	1900.	D. 101° 51′	Annual Change.	1900.
	″	″	″	″	″	″
Auwers' Bradley, 1755	50·16	...	...	51·67	...	...
Greenwich, 1880 ...	43·90	−0·050	42·90	54·79	+0·025	55·29
Radcliffe, 1890 ...	43·05	−0·053	42·52	54·23	+0·019	54·42
Nautical Almanac, 1895	43·18	−0·050	42·93	55·08	+0·024	55·20

The annual changes form the basis of a discussion of proper motions. The selection of ε *Orionis* is connected with this.

As to the places of the list for 1900. The *Nautical Almanac* places agree with the Greenwich, as they should. The Radcliffe and Greenwich have little or no average difference in A ; but the Greenwich *Polaris* distances are always in excess of the Radcliffe.

The same method could be applied directly to the results of observation corrected for refraction and aberration, and interpolated to the same time, if it were not for the insufficient accuracy of single observations of the fundamental stars.

As to the origin of ascension, secondary fundamental stars could be used, which would, in fact, be identical with the clock stars for the most part.

For *Polaris* additional material must be obtained, either by co-operation or observation out of the meridian, or both.

Errors in *Polaris* R.A. affect the results very little.

Assuming that sufficiently accurate places of *Polaris* can be got, the numbers obtainable through the calculations indicated will be directly comparable, as in the above example, without the intervention of precession or nutation.

Two methods are suggested for extra-meridian observations:—

(1) A fixed instrument for observation of *Polaris*, by means of which continuous observation of high accuracy would be possible.

(2) A transit instrument in a plane through the pole at right angles to the meridian.

The different historical values of the precession are summarised.

The values which have been attributed to the proper motion of *Polaris* in connection with the different precessions are exhibited in a comparable form in *Polaris* coordinates.

Places of the pole of the ecliptic and of the pole are given in *Polaris* coordinates for different dates; also a simple approximate method of dealing with the motion of the pole.

Suggestions are made for the tabulation of the functions which connect two systems of spherical coordinates, whose poles are not far apart, both in connection with the application of the above system to particular stars and as a more general question of spherical geometry.

Note.—The above is a summary of the chief contents of a paper, bearing the same title, which is to be placed in the library of the Society, together with a supplementary paper entitled "On the Star Survey by Fixed Coordinates in the Southern Hemisphere; with a Proposal for Cooperation."

On the Relation between Precession and Proper Motion.
By R. H. M. Bosanquet, F.R.S.

My former paper on a fixed system of star coordinates was based to some extent on the idea that, as changes in the adopted precession had been found desirable in the past, the same might happen in the future. I therefore saw with interest Mr. Stone's paper on the places of Eight Close Southern Polar Stars (*Monthly Notices*, LV., 299), where the argument is that the precession must be right because the residual proper motions are so small.

Now I have for some time been aware that the proper motion of *Polaris* is susceptible of diminution by an increase of the adopted precession; and it occurred to me in examining σ *Octantis*, as a possible pole of a southern star survey, to discuss the proper motion from this point of view. The result is that the proper motion of σ *Octantis* is also susceptible of diminution by an increase of the adopted precession; and the values of the increase of precession required to make the proper motion a minimum are similar in the two cases.

On the other hand 51 *Cephei* (Hev) requires a diminution of the precession, somewhat greater than the increase of the former cases, to make the proper motion a minimum.

These three stars form a system determining, by the combination of their minimum requirements, a precession differing not

very widely from that in use. And I propose to use them as illustrations in support of two propositions.

First, in the separate cases, certain large definite alterations in the precession cause no change in the magnitude of the resultant proper motions, while smaller alterations even diminish them.

Second, where the precession is considered with reference to a number of stars whose minimum proper motions require different precessions, large changes in the precession may produce very small changes in the average magnitude of the resultant proper motions within wide limits.

If these propositions are established, I think that no argument can be founded on the smallness of the proper motions to prove the precise exactness of the precession in use.

Every star has a minimum value for its proper motion, which occurs when the direction of the proper motion is along the great circle joining the star to the pole of the ecliptic. If the proper motion have any other direction, let it be resolved along, and at right angles to that great circle. Then a value of the precession can always be found, which shall absorb the component at right angles to the great circle, leaving the component along the circle as an irreducible minimum.

The process pursued in evaluating these quantities may be sketched as follows :—

A_ϵ is the angle at the star between any origin and the pole of the ecliptic.

A the angle between the same origin and the direction of the proper motion.

$A_\epsilon - A$ the inclination of the proper motion to the great circle through the star and the pole of the ecliptic.

$d\Sigma''$ is the resultant proper motion.

$d\Sigma'' \cos (A_\epsilon - A)$ component along circle to pole of ecliptic.

$d\Sigma'' \sin (A_\epsilon - A)$ component at right angles to circle.

D is the angular distance from the star to the pole of the ecliptic.

$\dfrac{d\Sigma'' \sin (A_\epsilon - A)}{\sin D}$ is the change of precession required for minimum proper motion.

$d\Sigma'' \cos (A_\epsilon - A)$ is the minimum proper motion.

In the case of *Polaris* the required data are taken from my paper "On a Fixed System of Star Coordinates," except the proper motion. I have reason to suppose, after examining Auwers' Bradley, that the discrepancy between the proper motion there found by comparison and the modern values, may be due chiefly

to the errors of the places of the Fundamenta. I therefore adopt the mean of the following values :—

Polaris.	$d\Sigma''$.	A of pole of motion, about Polaris, from ϵ Orionis.
	"	° ′ "
Auwers	0·0393	294 54 0
Greenwich, 1880 ...	0·0387	293 13 0
Radcliffe, 1890... ...	0·0381	293 59 0
Mean	0·0387	294 2 0
For direction of motion add		90 0 0
A from ϵ Orionis		24 2 0
A_ϵ from ϵ Orionis		184 38 0
D „		23 54 39

For the other stars a system of calculation was arranged.

Pole of the ecliptic.

A_ϵ, reckoned from the R.A. plane of the star, and D were found by formulæ of the same types as transformations I, II in my former paper.

Proper motion.

A, reckoned from the R.A. plane of the star, and $d\Sigma''$ were found as follows :—

$$(1)\quad \tan A = \frac{\sin \Delta d\alpha''}{d\Delta''} \qquad (2)\quad d\Sigma'' = \frac{d\Delta''}{\cos A}.$$

In the case of σ *Octantis* there was a slight difficulty. The proper motion assigned by Mr. Stone (*Monthly Notices*), lv. 299, is not there referred to any definite equinox. Assuming that the *Nautical Almanac* place for 1895 is correct (it is probably founded on Mr. Stone's calculations), I find the following proper motions for 1880 and 1895, through the medium of the *Polaris* places given in my paper on the "Star Survey by Fixed Coordinates in the Southern Hemisphere."* I now compare these proper motions with Mr. Stone's from the above reference :—

σ Octantis.	R.A.	N.P.D.
	"	"
1880	+1·049	+0·022
1895	+1·216	+0·020
Stone	+1·125	+0·02

* The *Polaris* proper motion was allowed for with the value given above, as well as the proper motion of ϵ *Orionis* in ascension, which is $-0''·027$ annually. Then the resultant of the remaining proper motion of the star was formed. The parallactic angle at the star between the *Polaris* and R.A. circles was found for the two dates, and the resultant proper motion resolved accordingly.

I used the numbers derived from the 1880–1895 comparison, taking everything with reference to 1880.

The proper motion and place of 51 *Cephei* (Hev.) were taken from the Greenwich Catalogue, 1880.

The value of ω employed for 1880 was

$$23^\circ\ 27'\ 17''{\cdot}40.$$

The following numbers were found—

	σ Octantis.	51 Cephei (Hev.).
A_ϵ	174° 3′ 0″	170° 20′ 44″
A	148 54 0	− 29 37 40
D	24 10 34	26 46 16
$d\Sigma''$	0 0 0·0257	0 0 0·0586

The following table exhibits the results (SE is the great circle from the star to the pole of the ecliptic) :—

Resolution of Proper Motions along and at Right Angles to SE; and Changes involved by Reduction of Proper Motion to Minimum.

	Resultant Proper Motion.	$A_\epsilon - A$.	Minimum Component along SE.	Numerical Diminution of Resultant.	Change in direction.	Component Perpendicular to SE.	Change of Precession required for diminution to minimum.
Polaris	0″·0387	160° 36′	−0″·0365	0″·0022	−19° 4′	+0″·0129	+0″·0317
σ Octantis	0·0257	25 9	+0·0233	0·0024	+25 9	+0·0109	+0·0267
51 Cephei (Hev.)	0·0586	199 58	−0·0551	0·0035	+19 58	−0·0200	−0·0445
						Mean	+0·0046

Resultants for Changes of Precession.

Change of Precession.	+0″·0317.	−0″·0445.
Polaris	0″·0365	0″·0478
σ Octantis	0·0233	0·0373
51 Cephei	0·0650	0·0551
Average of resultants 0″·0410	0·0416	0·0467

The reduction to the minimum changes the direction of the motion through the angle, "change in direction."

The application of twice the "change of precession required, &c.," would bring back the numerical resultants to their original values; but they would be turned through twice the angle, "change in direction."

The "change of precession required, &c.," is, for *Polaris* and σ *Octantis*, about twice the difference between Bessel's last

number (III. of my paper "On a Fixed System, &c.") and the modern value (Struve). It is about three times that difference for 51 *Cephei*.

Therefore, in the cases of *Polaris* and σ *Octantis*, the resultant, after diminishing, returns to the same numerical value, when the precession is increased by four times the difference (Struve—Bessel, III.). In the case of 51 *Cephei* the diminution of the precession, after which the resultant returns to its original value, is about six times that difference.

As far as the separate stars go, therefore, my first proposition is amply proved, viz., that, in the separate cases, large definite alterations in the precession produce no change in the magnitude of the resultant proper motions, while smaller alterations even diminish them.

We now pass to the average magnitude of the resultant proper motions : this is given for the precession in use and for the precessions corresponding to the minimum proper motions of *Polaris* and 51 *Cephei*. This covers a range of five times the difference (Struve Bessel III.), and the changes in the average magnitude of the resultants are very small.

There would, in a general determination, be cases of stars having their proper motions nearly at right angles to SE. In these cases only would a change of precession determine directly a commensurate change in the resultant proper motion.

I think I may now claim that my second proposition is established ; that, when the precession is considered with reference to a number of stars whose minimum proper motions require different precessions, large changes in the precession may produce very small changes in the average magnitude of the resultant proper motions within wide limits ; and, consequently, no argument can be founded on the smallness of the proper motions to prove the precise exactness of the precession in use.

Incidentally it appears that the best method of determining the precession from the proper motions of stars is the combination of the various precessions which determine the minimum proper motions of the stars concerned.

It is worth remark that circumpolar stars and other stars far from the ecliptic are unsuitable for the determination of precession, since the component at right angles to SE, by means of which the change of precession is found, is, in such cases, much less than the change of precession itself. Stars near the ecliptic are most suitable, as in these cases the component in question is nearly equal to the change of precession it determines.

Teneriffe : 1895 *May* 21.

Ephemeris for Physical Observations

Greenwich Noon.	Position-angle of ♃'s Axis. P	L−O	Diff.	B	Annual Parallax. Δ−L	Apparent Diameter. Equat. 2a	Defect.	Polar. 2b
1895.	°	°		°	°	″	″	″
Aug. 30	13·598	343·587	394	+1·051	−6·796	33·43	0·12	31·33
Sept. 1	13·750	343·981	389	1·035	7·026	33·53	·13	31·43
3	13·900	344·370	384	+1·019	−7·251	33·64	0·13	31·53
5	14·047	344·754	380	1·003	7·472	33·76	·14	31·64
7	14·192	345·134	375	0·988	7·688	33 88	·15	31·75
9	14·334	345·509	369	·972	7·899	34·00	·16	31·87
11	14·473	345·878	364	·957	8·104	34·13	·17	31·99
13	14·609	346·242	358	+0·941	−8·304	34·27	0 18	32·12
15	14·743	346·600	352	·925	8·498	34·41	·19	32·25
17	14·874	346·952	346	·910	8·687	34·55	·20	32·38
19	15·002	347·298	339	·894	8·870	34·70	·21	32·52
21	15·127	347·637	333	·879	9·046	34·85	·22	32·66
23	15·249	347·970	326	+0·864	−9·215	35·01	0·23	32·81
25	15·367	348·296	319	·849	9·378	35·17	·23	32·96
27	15·483	348·615	312	·834	9·534	35·33	·24	33·11
29	15·596	348·927	305	·819	9·683	35·50	·25	33·27
Oct. 1	15·705	349·232	297	·805	9·824	35·67	·26	33·44
3	15·811	349·529	288	+0·790	−9·957	35·85	0·27	33·61
5	15·914	349·817	280	·775	10·082	36·04	·28	33·78
7	16·014	350 097	272	·761	10·200	36·22	·29	33·95
9	16·110	350·369	263	·747	10·309	36·41	·29	34·13
11	16·202	350·632	255	·733	10·409	36·61	·30	34·31
13	16·291	350·887	245	+0·720	−10·500	36·81	0 31	34·50
15	16·376	351·132	236	·706	·583	37·01	·31	34·69
17	16·458	351·368	226	·693	·656	37·22	·32	34·89
19	16·536	351·594	216	·680	·719	37·43	·33	35·09
21	16·611	351·810	205	·668	·772	37·65	·33	35·29
23	16·682	352·015	196	+0·655	−10·815	37·87	0 34	35·49
25	16·749	352·211	185	·643	·848	38·09	·34	35·70
27	16·812	352·396	174	·631	·870	38·32	·34	35·91
29	16·872	352·570	162	·619	·881	38·55	·35	36·13
31	16·927	352·732	151	·608	·881	38·78	·35	36·34
Nov. 2	16·979	352·883	140	+0·597	−10·870	39·01	0·35	36·56

of Jupiter, 1895–96. By A. Marth.

Greenwich Noon.	Brightness in Star Magn.	Longitude of ☿'s Central Meridian. (877°·90) I.	(870°·27) II.	Corr. for Phase.	Light-time.	A−O	B
1895.	m	°	°	°	m	°	°
Aug. 30	−1·45	83·43	311·62	+0·20	49·833	336·7944	+1·2083
Sept. 1	−1·45	38·94	251·86	·21	49·674	336·9582	1·2002
3	−1·46	354·45	192·11	+0·23	49·509	337·1220	+1·1921
5	−1·47	309·97	132·37	·24	49·339	337·2858	1·1840
7	−1·47	265·49	72·63	·26	49·164	337·4495	1·1759
9	−1·48	221·03	12·91	·27	48·984	337·6132	1·1678
11	−1·49	176·57	313·19	·29	48·798	337·7769	1·1597
13	−1·50	132·13	253·48	+0·30	48·608	337·9405	+1·1515
15	−1·50	87 69	193·78	·31	48·412	338·1041	1·1434
17	−1·51	43·26	134·09	·33	48·212	338·2676	1·1353
19	−1·52	358·83	74·41	·34	48·007	438.4311	1·1272
21	−1·53	314·42	14·74	·36	47·798	338·5945	1·1190
23	−1·54	270 02	315·07	+0·37	47·585	338·7579	+1·1108
25	−1·55	225·63	255·42	·38	47·367	338·9212	1·1027
27	−1·56	181·24	195·77	·40	47·146	339·0845	1·0945
29	−1·57	136·87	136·14	·41	46·920	339·2477	1·0863
Oct. 1	−1·58	92·50	76·51	·42	46·691	339·4109	1·0781
3	−1·59	48·15	16·89	+0·43	46·459	339·5741	+1·0699
5	−1·60	3·80	317·29	·44	46·224	339·7372	1·0617
7	−1·61	319·47	257·69	·45	45·985	339·9003	1·0535
9	−1·62	275·14	198·11	·46	45·743	340·0633	1·0452
11	−1·63	230·83	138·53	·47	45·499	340·2263	1·0370
13	−1·64	186·53	78·96	+0·48	45·252	340·3892	+1·0288
15	−1·66	42·23	19·41	·49	45·003	340·5521	1·0206
17	−1·67	97·95	319·87	·49	44·752	340·7149	1·0124
19	−1·68	53·68	260·33	·50	44·499	340·8777	1·0042
21	−1·69	9·42	200·81	·50	44·244	341·0404	0·9959
23	−1·70	325·17	141·30	+0·51	43·988	341·2031	+0·9876
25	−1·72	280·93	81·80	·51	43·731	341·3657	·9794
27	−1·73	236·70	22·31	·51	43·473	341·5283	·9711
29	−1·74	192·48	322·83	·52	43·214	341·6909	·9628
31	−1·75	148·28	263·37	·51	42·955	341·8534	·9545
Nov. 2	−1·77	104·09	203·91	+0·51	42·696	342 0159	+0·9462

Greenwich Noon.	Position-angle of ♃'s Axis. P	L−O.	Diff.	B	Annual Parallax. A−L.	Apparent Diameter. Equat. 2a	Defect.	Polar. 2b
1895.	°	°		°	°	″	″	″
Nov. 4	17·026	353·023	128	·586	·847	39·25	·35	36·79
6	17·070	353·151	117	·575	·813	39·49	·35	37·01
8	17·109	353·268	104	·565	·767	39·73	·35	37·24
10	17·145	353·372	92	·554	·709	39·98	·35	37·47
12	17·176	353·464	79	+0·545	−10·638	40·22	0·35	37·70
14	17·203	353·543	67	·536	·555	40·47	·34	37·93
16	17·225	353·610	54	·527	·460	40·72	·34	38·17
18	17 244	353·664	40	·519	·352	40·97	·33	38·40
20	17·258	353·704	27	·511	·230	41·22	·33	38 63
22	17·268	353·731	14	+0·503	−10 095	41·47	0·32	38·87
24	17·273	353·745	2	·496	9·947	41·72	·31	39·10
26	17·274	353·747	12	·489	9·787	41·97	·31	39·34
28	17·271	353·735	25	·483	9·613	42·22	·30	39·57
30	17·263	353·710	38	·477	9·426	42·46	·29	39·80
Dec. 2	17·251	353·672	52	+0·471	−9·226	42·71	0·28	40·03
4	17·235	353·620	64	·466	9·013	42·95	·27	40·25
	17·215	353·556	77	·461	8 787	43·19	·25	40·48
8	17·190	353 479	91	·457	8·548	43·42	·24	40·70
	17·160	353·388	103	·453	8·296	43·65	·23	40·91
12	17·127	353·285	115	+0·449	−8 031	43·88	0·22	41·12
14	17·089	353·170	128	·446	7·754	44·10	·20	41·33
16	17·047	353·042	140	·444	7·465	44·31	·19	41·53
18	17·001	352·902	152	·442	7·163	44·52	·17	41·73
20	16·950	352·750	163	·440	6·850	44·72	·16	41·92
22	16·896	352·587	174	+0·439	−6·526	44·91	0·15	42·10
24	16·838	352·413	184	·438	6·191	45·10	·13	42·27
26	16 776	352·229	194	·438	5·845	45·28	·12	42·44
28	16·711	352·035	204	·438	5·490	45·45	·10	42·60
30	16·642	351·831	213	·438	5·125	45·60	·09	42·74
1896.								
Jan. 1	16·570	351·618		+0·439	−4·751	45·75	0·08	42·88

Greenwich Noon.	Brightness in S ar Magn.	Longitude of ℧'s Central Meridian. (877°·90) I.	(870°·27) II.	Corr. for Phase.	Light-time.	A − O	B
1895.	m	°	°	°	m	°	°
Nov. 4	−1·78	59·91	144·47	·51	42·438	342·1783	·9379
6	−1·79	15·73	85·04	·51	42·179	342·3407	·9296
8	−1·81	331·57	25·62	·50	41·922	342·5030	·9213
10	−1·82	287·43	326·21	·50	41·665	342·6653	·9130
12	−1·83	243·29	266·81	+0·49	41·410	342·8276	+0·9047
14	−1·84	199·17	207·42	·49	41·156	342·9898	·8964
16	−1·86	155·05	148·05	·48	40·905	343·1520	·8881
18	−1·87	110·95	88·69	·47	40 656	343·3141	·8798
20	−1·88	66·86	29·34	·46	40·409	343·4761	·8715
22	−1·90	22·78	330·00	+0·44	40·165	343·6381	+0·8631
24	−1·91	338 71	270·67	·43	39·925	343·8001	·8548
26	−1·92	294·66	211·35	·42	39·688	343·9620	·8465
28	−1 94	250·61	152 04	·40	39·455	344·1239	·8382
30	−1·95	206·58	92·74	·39	39·226	344·2857	·8298
Dec. 2	−1·96	162·55	33·46	+0·37	39·002	344·4475	+0·8214
4	−1·97	118·54	334·18	·35	38·783	344·6093	·8131
6	−1·99	74·53	274·91	·34	38·569	344·7710	·8047
8	−2·00	30·53	215·66	·32	38·361	344·9327	·7963
10	−2·01	346·55	156·41	·30	38·158	345·0943	·7879
12	−2·02	302·57	97·17	+0·28	37·962	345·2559	+0·7795
14	−2·03	258·60	37·94	·26	37·772	345·4174	·7711
16	−2·04	214·64	338·72	·24	37·589	345 5789	·7627
18	−2·05	170·69	279·51	·22	37·413	345·7404	·7543
20	−2·06	126·74	220·30	·20	37·244	345·9018	·7459
22	−2·07	82·80	161·10	+0·18	37·083	346·0632	+0·7375
24	−2·08	38·87	101·91	·17	36·931	346·2245	·7291
26	−2·09	354·94	42·72	·15	36·787	346·3858	·7207
28	−2·10	311·02	343·54	·13	36·651	346·5470	·7123
30	−2·11	267·10	284·36	·11	36·524	346·7082	·7039
1896							
Jan. 1	−2·11	223·19	225·18	+0·10	36·407	346·8694	+0·6954

The angle L−O+180° is the jovicentric longitude of the Earth, reckoned in the assumed plane of *Jupiter's* equator from O, the point of the vernal equinox of *Jupiter's* northern hemisphere, or the point of the descending node of the planet's equator on its orbit. B denotes the jovicentric latitude of the Earth above *Jupiter's* equator, Λ−L the difference between the longitudes of the Sun and of the Earth.

The apparent diameters of the disc of *Jupiter* depend on Barnard's measurements, published in No. 325 of *Gould's Astron. Journal*, allowance being made for the Earth's latitude above the plane of *Jupiter's* equator. If cos ϵ_0 denotes the ratio of the polar axis of the planet's spheroid to its equatorial diameter, and cos ϵ the ratio of the apparent diameters of the disc as seen from a distant point in latitude B, the connection between cos ϵ and cos ϵ_0 is found from the equation $\sin \epsilon = \sin \epsilon_0 \cos B$.

Hence

$$\cos \epsilon = \cos \epsilon_0 \sqrt{1 + \tan^2 \epsilon_0 \sin^2 B}$$
$$= \cos \epsilon_0 \,.\, \sec \nu, \text{ if } \tan \nu = \tan \epsilon_0 \sin B.$$
$$\cos \epsilon_0 = \cos \epsilon \sqrt{1 - \tan^2 \epsilon \tan^2 B}$$
$$= \cos \epsilon \,.\, \cos \gamma, \text{ if } \sin \gamma = \tan \epsilon \tan B.$$

The formulæ for finding the differences of tangents to the limbs in right ascension and declination and the defects of illumination are given in vol. xlv., p. 508.

The longitudes of *Jupiter's* central meridian are computed with the values of the rates of rotation and of the zero meridians adopted in the preceding ephemerides. The addition of the "Corr. for Phase" gives the longitudes of the meridian which bisects the illuminated disc. The following is a list of Greenwich mean times when the adopted zero meridians in the assumed two systems of longitudes will pass the middle of the illuminated disc :—

	I. (877°·90)	II. (870°·27)		I. (877°·90)	II. (870°·27)
1895.	h m	h m	1895.	h m	h m
Aug. 30	17 24·0	21 11·3	Sept. 10	19 9·9	15 21·2
31	22 55·8	17 2·8	11	14 51·0	21 8·5
Sept. 1	18 37·0	22 50·1	12	20 22·8	17 0·0
2	14 18·1	18 41·7	13	16 3·9	22 47·3
3	19 49·9	14 33·2	14	21 35·6	18 38·8
4	15 31·1	20 20·5	15	17 16·8	14 30·3
5	21 2·9	16 12·1	16	22 48·5	20 17·6
6	16 44·0	21 59·4	17	18 29·6	16 9·1
7	22 15·8	17 50·9	18	14 10·8	21 56·3
8	17 57·0	23 38·2	19	19 42·5	17 47·8
9	23 28·7	19 29·7	20	15 23·6	23 35·0

1895.	I. (877°·90) h m	II. (870°·27) h m	1895.	I. (877°·90) h m	II. (870°·27) h m
Sept. 21	20 55·3	19 26·5	Oct. 21	9 34·2	14 18·3
22	16 36·4	15 18·0		19 24·8	24 14·0
23	22 8·1	21 5·2	22	15 5·8	10 9·7
24	17 49·2	16 56·7		24 56·3	20 5·3
25	13 30·6	12 48·2	23	10 46·8	15 56·7
	23 20·9	22 43·9		20 37·3	25 52·4
26	19 2·0	18 35·4	24	16 18·3	11 48·1
27	14 43·1	14 26·9	25	11 59·4	17 35·2
28	0 33·7	0 22·6	26	17 30·9	13 26·5
	20 14·8	20 14·0	27	13 11·9	9 17·9
29	15 55·9	16 5·5		23 2·4	19 13·5
30	21 27·6	21 52·7	28	18 43·4	15 5·0
Oct. 1	17 8·7	17 44·1	29	14 24·4	10 56·3
2	12 49·8	13 35 6		24 14·9	20 52·0
	22 40·3	23 31·3	30	19 55·9	16 43·4
3	18 21·4	19 22·8	31	15 36·9	12 34·7
4	14 2·5	15 14·2	Nov. 1	11 17·9	8 26·1
5	19 34·1	21 1·4		21 8·4	18 21·8
6	15 15·2	16 52·8	2	16 49·4	14 13·1
7	10 56·3	12 44·3	3	12 30·4	10 4·4
	20 46·8	22 40·0		22 20·9	20 0·1
8	16 27·9	18 31·4	4	8 11·4	15 51·4
9	12 9·0	14 22·8		18 1·9	25 47·1
	21 59·5	24 18 6	5	13 42·9	11 42·8
10	17 40·6	20 10·0		23 33·4	21 38·4
11	13 21·6	16 1·4	6	9 23·9	7 34·1
	23 12·2	25 57·1		19 14·3	17 29·8
12	18 53·2	21 48·5	7	14 55·3	13 21·1
13	14 34·3	17 40·0		24 45 8	23 16·8
14	10 15·4	13 31·4	8	10 36·3	9 12·4
	20 5 9	23 27·1		20 26·8	19 8·1
15	15 46·9	19 18·5	9	16 7·7	14 59·4
16	21 18·5	15 9·9	10	1 58 2	0 55·1
17	16 59 6	20 57·0		11 48·7	10 50·7
18	12 40·6	16 48·4		21 39 2	20 46·4
19	18 12·2	12 39·8	11	17 20·1	16 37·7
20	13 53·2	8 31·2	12	13 1·1	12 29·0
	23 43 7	18 26·9		22 51·6	22 24·7

1895.	I. (877°·90) h	m	II. (870°·27) h	m	1895.	I. (877°·90) h	m	II. (870°·27) h	m
Nov. 13	8	42·0	8	20·3	Dec. 4	6	35·4	10	37·8
	18	32·5	18	16·0		16	25·9	20	33·4
14	14	13·5	14	7·3	5	12	6·8	6	29·0
15	0	3·9	0	2·9		21	57·2	16	24·6
	9	54·4	9	58·6	6	7	47·6	12	15·8
	19	44·9	19	54·2		12	16	Sat. III. Sh.	
16	15	25·8	15	45·5		17	38·1	22	11·4
17	11	6·8	11	36·8	7	13	19·0	8	7·0
	20	57·2	21	32·5		23	4·4	18	2·7
18	16	38·2	17	23·8	8	8	59·8	13	53·9
19	12	19·1	13	15·1		18	50·3	23	49·5
	22	9·6	23	10·7	9	14	31·1	9	45·1
20	8	0·1	9	6·3		18	19	Sat. IV. Sh.	
	17	50·5	19	2·0		24	21·6	19	40·7
21	13	31·5	14	53·3	10	10	12·0	5	36·3
	23	21·9	24	48·9		20	2·4	15	31·9
22	9	12·4	10	44·5	11	5	52·9	11	23·2
	19	2·8	20	40·2		15	43·3	21	18·8
23	0	20	Sat. IV. Shadow crossing central Meridian.		12	11	24·2	7	14·4
						21	14 6	17	10·0
	14	43·8	16	31·5	13	7	5·0	13	1·2
24	10	24·7	12	22·7		16	14	Sat. III. Sh.	
	20	15·1	22	18·4		16	55·4	22	56·8
25	15	56·0	18	9·6	14	12	36·3	8	52·4
26	11	36·9	14	0·9		22	26·7	18	48 0
	21	27·4	23	56·5	15	8	17·2	14	39·2
27	7	17·8	9	52·1		18	7·6	24	34·8
	17	8·3	19	47·8	16	13	48 4	10	30·4
28	12	49·2	15	39·0		23	38·9	20	26·0
29	8	30·1	11	30·3	17	9	29·3	6	21·6
	18	20·5	21	25·9		19	19·7	16	17·2
30	14	1·5	17	17·2	18	5	10·1	12	8·4
Dec. 1	9	42·3	13	8·4		15	0·6	22	4·0
	19	32·8	23	4·0	19	10	41·4	7	59·6
2	5	23·4	8	59·7		20	31·8	17	55·2
	15	13·7	18	55·3	20	16	12·7	13	46·4
3	10	54·6	14	46·5		20	14	Sat. III. Sh.	
	20	45·0	24	42·1	21	11	53·5	9	37·6

	I. (877°·90)	II. (870°·27)		I. (877°·90)	II. (870°·27)
1895	h m	h m	1895	h m	h m
Dec. 21	21 43·9	19 33·2	Dec. 26	19 48·9	18 40·2
22	7 34·3	5 28·8	27	15 29·7	14 31·4
	17 24·7	15 24·3	28	11 10·5	10 22·6
23	13 5·6	11 15·5		21 0·9	20 18·2
	22 56·0	21 11·1	29	6 51·3	6 13·8
24	8 46·4	7 6·1		16 41·7	16 9·4
	18 36·8	17 2·3	30	12 22·6	12 0·5
25	14 17·6	12 53·5		22 13·0	21 56·1
26	9 58·5	8 44·7	31	8 3·4	7 51·7
	12 18	Sat. IV. Sh.		17 53·8	17 47·3

(*To be continued in the Supplementary Number.*)

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Correction.

Mr. Barnard's attention has been called by Mr. T. J. Moore, of Doncaster, England, to an error in the value given in Mr. Barnard's paper on *Saturn*, *Monthly Notices*, vol. lv. p. 377.

For the polar compression

$$C = \frac{E-P}{E}$$

should be $\frac{1}{12\,35}$

instead of $\frac{1}{11{\cdot}44}$.